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AND

JOURNAL OF SCIENCE.

CONDUCTED BY

SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.

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AND

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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

VOL. XV.—FIFTH SERIES.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condât,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonum.

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ERRATA IN VOL. XIV.

Page 297, line 32, *for* $A+u'$ is $A+u'\{\&c.$

read $A+u'$ is $A+u'-\{\&c.$

— — line 33, *for* $u'u''-+\frac{2}{n}(B-A)$

read $u'+u''-\frac{2}{n}(B-A)$

PLATES.

- I. Illustrative of Mr. J. LeConte's Paper on the Apparent Attractions and Repulsions of Small Floating Bodies.
- II. Illustrative of Mr. W. Baily's Paper on the Spectra formed by Curved Diffraction-gratings.
- III. Illustrative of Mr. S. P. Langley's Paper on the Selective Absorption of Solar Energy.
- IV. Illustrative of Mr. J. T. Riley's Paper on Capillary Phenomena.
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- VI. Illustrative of Mr. L. Wright's Paper on Optical Combinations of Crystalline Films.
- VII. Illustrative of Mr. J. Rand Capron's Paper on the Auroral Beam of November 17, 1882.
- VIII. & IX. Illustrative of Professors Ayrton and Perry's Paper on winding Electromagnets.

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[FIFTH SERIES.]

JANUARY 1883.

I. *Researches on the Passage of Electricity through Rarefied Air.* By E. EDLUND*.

§ 1.

IN a previous memoir† I brought together, in order to compare them with each other, the researches made at different times by various physicists on the passage of electricity through rarefied gases, and I endeavoured, *inter alia*, to demonstrate that *vacuum* is a conductor of electricity, or at least opposes but an inconsiderable resistance to its propagation. This result is in flagrant contradiction to the hitherto generally received opinion that *vacuum* is a perfect insulator. The reason that an electric current cannot traverse the Torricellian vacuum does not depend on *vacuum* itself being an insulator, but on the fact that there exists at the point of passage between the electrodes and the rarefied gas an obstacle to the propagation of electricity, and that this obstacle is augmented in proportion as the air is rarefied. As soon as the gas has been sufficiently rarefied, the obstacle in question has become so powerful that the current is incapable of surmounting it, and, consequently, of traversing the rarefied air. According to

* Translated from a copy, communicated by the Author, of No. 1, vol. xx. of the *Kongliga Svenska Vetenskaps-Akademiens Handlingar*.

† Memoirs (*Handlingar*) of the Royal Academy of Sciences of Sweden, xix. no. 2; *Ann. de Chim. et de Phys.* xxiv. p. 199 (1881); *Phil. Mag.* [5] xiii. p. 1 (1882); *Wied. Ann.* xv. p. 514 (1882).

this interpretation, then, it is not at all the resistance of the rarefied gas that forms an obstacle to the passage of the current, but the obstacle must be sought at the point of passage between the electrodes and the rarefied gas. The current, therefore, if it could be introduced into the vacuum without the aid of the electrodes, would be able to pass through the vacuum without difficulty. The experiments which have up to the present been made on the passage of electricity through rarefied gases are, I am deeply convinced, in favour of the correctness of this explanation. Besides, several physicists have been led to declare that there really does exist, at the point of passage between the electrodes and the rarefied gas, a special obstacle to the propagation of the current. But, in my opinion, they have not understood the true nature of that obstacle, nor have they attempted to demonstrate that its magnitude increases with the rarefaction of the gas; moreover they have not attributed to it the high importance which belongs to it.

The result of the above-mentioned memoir, so far as it regards the subject of the present, may be summed up as follows:—If r is the obstacle to the propagation of electricity which is situated at the point of passage between the electrodes and the gas, r_1 the electric resistance in a column of gas of unit length, and l the distance between the electrodes, $r + r_1 l$ will be the sum of the resistance which the electricity must overcome in order to pass from one electrode to the other. Of these values, r increases continually in proportion as the gas is rarefied, while r_1 during the same time undergoes incessant diminution. From a judicious interpretation of the experiments which have been made, especially those of Hittorf, it follows that the augmentation of the first of these quantities and the diminution of the other continued until the gas had arrived at the greatest rarefaction it was possible to obtain by means of the mercury pump employed. In the space exhausted of air, r acquires such a value that it is impossible for the current to surmount it. If, then, the current cannot traverse a vacuum, it is not because the value r_1 of the resistance has become too great, but because r is augmented to such a degree that the current is incapable of surmounting it. Several properties of gases with respect to the passage of electricity show that this interpretation of the resistance which they oppose to its propagation is really the only true one.

In my memoir before mentioned, I have formulated the opinion that the principal obstacle encountered by the electric current at the surface of passage between the electrodes and the rarefied gas is due to an electromotive force producing a

current in the opposite direction to the principal current. It will be experimentally proved, further on, that this is indeed the case, and that, at least when the rarefaction has reached a certain limit, this force undergoes continuous augmentation if the rarefaction be carried still further. But it will be convenient, before passing to the statement of the experiments performed, to call attention to the following circumstances:—

I have demonstrated, in my unitary theory of electricity, that the resistance undergone by an electric current in passing through a solid or liquid conductor will be proportional to the intensity of the current*. As I have shown in that memoir, the supposition hitherto regarded as true, that the resistance is independent of the intensity of the current, really leads to absurd conclusions; while the supposition that the resistance of the conductor is proportional to the intensity of the current is found to be in complete conformity with the experiments hitherto made, and leads to results ratified by experience. What has just been said applies to solid and liquid bodies, but not to gases at ordinary temperature, as the following consideration, among others, will show:—In order that a current started by an electromotor may be able to pass through a solid or liquid conductor, there is no need of a determined electromotive force in the electromotor. However slight the force may be, the current will always pass, although of course it becomes less intense in proportion as the electromotive force is little or the resistance of the conductor is great. If in all cases the current is able to traverse the conductor, that evidently depends on the effective resistance being proportional to the intensity of the current, and hence that resistance is least when the intensity of the current is least. On the contrary, in order that the current may be able to pass through a gaseous body, a determined electromotive force is necessary, or, what amounts to the same, it is necessary that the electromotor be in a condition to produce a certain tension on the electrodes. If the electromotive force is below this limit, experiment shows that gas may be regarded as a perfect insulator. From this it evidently follows that the resistance of gases cannot be proportional to the intensity of the current. Now, if it be admitted that the electric resistance of gases is independent of the intensity of the current, the properties of which, according to the experiments of the last few years†,

* "Théorie des phénomènes électriques," *K. Svenska Vet.-Akad. Handlingar*, t. xii. no. 8; *Pogg. Ann.* cxlviii. p. 421 (1873); *Phil. Mag.* [4] xlvi. p. 204.

† *Bihang till K. Svenska Vet.-Akad. Handlingar*, vi. no. 7 (1881); *Wied. Ann.* xv. p. 165 (1882); *Phil. Mag.* [5] xiii. p. 200.

gases give proof when traversed by the current can be explained with the greatest facility, namely:—that the amount of heat generated is proportional to the intensity of the current, not to the square of that intensity; that the resistance in a column of gas is independent of the cross section of the column; that the difference between the electroscopic tensions of two points situated at a distance from one another is proportional to the resistance between the same points, not to that resistance multiplied by the intensity of the current, &c. Every thing, consequently, leads to the conclusion that the electric resistance of gases is independent of the intensity of the current, but of course only on the supposition that the current does not sensibly modify the composition, temperature, or density of the gas.

Let i be the current-intensity in a closed circuit composed exclusively of solid and liquid parts, E the electromotive force of the electromotor, and n the quantity of surface of the latter, L the total length of the circuit, and r its resistance at unit intensity of the current; we shall have for the determination of that intensity the differential equation *

$$L \frac{di}{dt} = nE - nri.$$

If we integrate this equation, we shall get, for the case in which the current has been closed a sufficient time for its intensity to have become constant,

$$i = \frac{E}{r};$$

that is to say, the known law of Ohm.

When, on the contrary, the circuit contains in addition a gaseous conductor whose resistance is R , the differential equation becomes

$$L \frac{di}{dt} = nE - nR - nri,$$

of which the integral is

$$i = \frac{E - R}{r}.$$

Therefore the resistance R of the gas has its place in the numerator and not in the denominator as, without closer examination, one would be inclined to place it, according to the usual formula of Ohm. It is seen, then, as a consequence of what has been said above, that E must be greater than R in order that it may be possible for a current to arise.

* Pogg. *Ann.* cxlviii. p. 421; Phil. Mag. [4] xlv. p. 206.

§ 2.

The apparatus employed in the experiments were:—a Holtz electrophorus with a double rotatory disk, the crank of which was turned, with a constant velocity, as a rule once in two seconds; a reflecting galvanometer of a construction previously described*, specially applicable to electric discharges, the coil of which consisted of forty turns of copper wire 1·3 millim. in thickness, surrounded with gutta percha; a Töpler's mercury air-pump of improved construction by Bessel-Hagen†, as well as a Ruhmkorff induction-apparatus, which, however, could only give very short sparks in a space filled with air. The intensity of the induced current was measured by a magnetometer, the deflections of which were read with the aid of a telescope and scale.

I proved, several years since, by experiment, that there exists in the voltaic arc, as well as in the electric spark, an electromotive force sending a current in the opposite direction to that which calls forth the electromotive force‡. In order to distinguish it from other currents, it received the name of disjunction-current. The above-mentioned result has been verified by Sundell, in regard to the electric spark, by means of a series of detailed experiments§. In those experiments the electric spark was formed in a space filled with air. An investigation which I subsequently made showed that the force discovered by me in a space filled with air existed also when the air was rarefied; but the resources at my disposal on that occasion did not permit me to carry the rarefaction far enough; moreover it was not in the programme of the investigation to devote at that time more attention to this matter||. I will therefore now show, pursuing the same method as that which I employed in 1868, the manner in which the electromotive force is modified with the density of the air. Of the experi-

* *Öfversigt af Kongl. Vetenskaps-Akademiens Förhandlingar*, 1868; Pogg. *Ann.* cxxxvi. (1862); *Ann. de Chim. et de Phys.* [4] xvii.; *Phil. Mag.* [4] xxxviii. p. 169.

† *Wied. Ann.* xii. p. 425 (1881).

‡ *Öfvers. af Kongl. Vet.-Akad. Förhandl.* 1867 & 1868; Pogg. *Ann.* cxxxi., cxxxiv. pp. 250, 337; *Phil. Mag.* [4] xxxvi. & xxxvii.

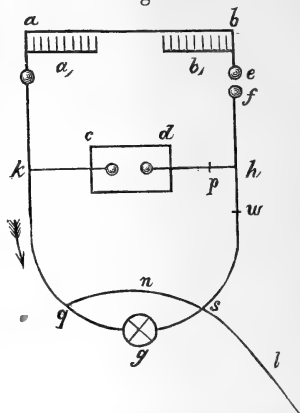
§ Pogg. *Ann.* cxlv.

|| In a communication to the Paris Academy of Sciences (*Comptes Rendus* for 1881, t. xcii. p. 710), M. Le Roux states that my investigation on the voltaic arc has merely led to this result—that the arc does not behave exclusively like a simple resistance; and he then gives a proof that there really exists an electromotive force. Now this proof is identical with that which I employed in 1867 to demonstrate the existence of the same force.

ments made, in order to economize space I shall cite only those which are absolutely necessary to furnish a complete demonstration*.

Fig. 1.

In fig. 1, ab represents the disk of the electrophorus, a_1 and b_1 the two receiving combs, of which a_1 is in metallic connexion with the point k , and b_1 with the brass ball e . In the vicinity of this latter another brass ball, f , is placed, from which an electrode goes to the point h . The space in which the air is to be rarefied is represented by $c d$, and is connected with the points h and k . From these points also electrodes go to the galvanometer g , in front of which the



wires are joined by a bridge n , which presents a suitable resistance: the purpose of this bridge will be seen further on. l is a copper wire connected to earth, and carries away the static electricity which has possibly been left in the galvanometer after the discharges, and might produce an electroscopic effect upon the magnetized needle.

When the disk of the electrophorus is put into rotation, the sparks spring between the balls e and f , and the magnetized needle of the galvanometer g is deflected. If the rotation be continued for a sufficient time, the deflection will become constant. The arrival of that moment, however, was not always waited for; but the amount of the deflection was determined by observing the degrees of the scale to which the needle returned in its oscillations; then a mean of those degrees was taken. Several such means were taken for each determination. If (as indicated in the figure by an arrow) the positive current passes from the receiving comb a_1 , this current divides at k , after which a portion of it traverses the space of rarefied air, while, on the other hand, a certain quantity passes through the bridge n , and the remainder through the helix of the galvanometer. But when the conduction is interrupted between the point k and the rarefied air-space, the entire current passes to g , and is there divided between the

* The observations were carried out, under my direction, by MM. S. Arrhenius and C. Mebius, Candidates in Philosophy of the University of Upsal, and Th. Homen, Candidate in Philosophy of the University of Helsingfors (Finland). Usually each observation was repeated three times, which permitted the observers to control each other.

galvanometer and the bridge. All the series of observations agreed in this:—that if the conduction between k and the rarefied-air space was interrupted, and consequently the entire discharge passed to g and was there divided between the bridge and the galvanometer to arrive at the ball f , the deflection did not amount to more than 1 or 1·5 division of the scale; but if the discharge divided at k and part of it passed through the rarefied air, the deflection might amount to 50 scale-divisions—although a lower figure might have been expected, seeing that in this case a smaller portion of the discharge passed through the galvanometer.

The question now is, to what cause must we ascribe the circumstance that the deflections were several times greater in one case than in the other? Perhaps the first thought that will occur to us is that the discharge is oscillatory. One might then say:—“At the first swing positive electricity is discharged, without dividing at k , through the bridge n and the galvanometer to arrive at the ball f ; when, at the second swing (a little weaker than the first), the electricity returns, it takes its path to h exclusively through the rarefied-air space; at the third swing it again passes exclusively through the galvanometer and the bridge, while at the fourth it is discharged only through the rarefied-air space, and so on. In this way the deflections would be greater when k is in metallic communication with the rarefied air than when the communication between them is interrupted.” But it will readily be perceived that an oscillatory discharge of this kind is absolutely impossible. The same direction and the same amount of deflection have been observed, whether the point of interruption between the balls e and f was near to b_1 , as shown in the figure, or placed near to a_1 . It can therefore be said that every thing is symmetric around the rarefied-air space, and hence there is no valid reason for the electricity, in passing from left to right, to be brought to choose another path than that which it takes when it returns from right to left. Further on, moreover, will be found some proofs demonstrating that the oscillatory discharge cannot be the cause of the great deflections mentioned above.

Others will perhaps be disposed to assume that the reason of the great deflections which take place when the point k is connected with the rarefied-air space, is the rise of induction-currents, at the time of the discharge, in the helix of the galvanometer or at some other point of the closed circuit, and that to these currents we must attribute the above-mentioned deflections. Now, further on will likewise be given proofs that the great deflections have just as little to do with induced

currents. We are therefore compelled to admit that there must be in the electric spark a proper electromotive force sending a current in the inverse direction of the discharge. As I had before demonstrated that the voltaic arc is the seat of an electromotive force, it was in reality easy to foresee that such a force must also exist in the electric spark.

Experiment 1. At the two ends of a glass tube of 20 millim. diameter and 60 millim. length, aluminium electrodes were fixed. The distance between the extremities of these wires amounted to 5 millim. A glass tube of the same dimensions contained brass electrodes, the extremities of which were furnished with balls of the same metal; the distance between the nearest points of the latter amounted also to 5 millim. The tubes were connected with the mercury air-pump; and consequently the air had the same density in both. The distance between the balls *e* and *f* (fig. 1) of the electrophorus was 10 millim. At *w* a rheostat, presenting a resistance of 7.75 ohms, was placed. This rheostat was composed of a fine German-silver wire wound round four glass columns, each turn of the spiral being sufficiently distant from the neighbouring turn to prevent any appreciable induction at the time of the discharge. In this way the following deflections were obtained:—

Pressure of the air, in millimetres.	Deflections, in scale-divisions, with	
	the brass balls.	the aluminium poles.
561.7 . . .	14.1	11.8
353.0 . . .	9.3	10.3
249.0 . . .	8.8	7.7
138.9 . . .	5.2	4.6
72.2 . . .	3.4	2.9
29.5 . . .	1.7	3.0
6.5 . . .	1.5	3.8
4.4 . . .	3.0	8.2
2.0 . . .	13.3	16.3

When the communication between *k* and the rarefied-air space was interrupted, so that the entire discharge of the electrophorus must divide itself between the bridge *n* and the galvanometer, a deflection of 1.5 scale-division was obtained. In the experiments cited, a portion of the discharge passed through the rarefied-air space; so that the deflection proceeding from the quantity of electricity which came direct from the electrophorus could scarcely amount to more than one division of the scale. If we subtract this from the above-given

numbers, we get the deflections produced by the electromotive force existing in the spark.

The resistance of a column of gas being independent of the cross section of the column, but being necessarily proportional to its length, the resistance of the spark may be expressed by $r_1 l$, in which term r_1 denotes the resistance of unit length, and l the length. If we represent by e the electromotive force of the spark, and by m the resistance of the rest of the closed circuit, the deflections obtained must, according to what precedes, be (after deducting one scale-division) proportional to $\frac{e - r_1 l}{m}$. The experiments recapitulated above show that the

difference $e - r_1 l$ diminishes when the pressure of the air falls from 562 to about 29 millim., but that afterwards it increases again in proportion as the pressure descends to 2 millim. Now the preceding experiments show only the way in which the difference between e and $r_1 l$ varies with the pressure, and not the modifications undergone by each of these quantities.

Before proceeding further, there is reason to pay attention to the following circumstances:—When an electric spark is formed between metal electrodes in air of great density, the temperature of the circuit of the spark rises, as is known, to such a degree that the metals are carried in the gaseous state from one electrode to the other. The air is therefore considerably heated, and mixed besides with metallic vapours. Now we know from Becquerel's researches, confirmed by Hit-
torf's experiments, that at high temperature gases become relatively good conductors. It is consequently impossible to deduce, from experiments performed at a considerable density of the air, what would have been the value of its conductivity at the same density if the air had kept its temperature unchanged. With a greater density of the air, too, the electrodes are torn by the current; and as it is then very probable (as I have endeavoured to demonstrate in my first researches upon the electromotive force in question) that e in part depends on this, neither is it possible to draw, from experiments made with a considerable density of the air, any sure conclusions concerning the dependence of that force on the density of the air when the tearing above mentioned does not take place. If, on the contrary, the air is highly rarefied, it, as perfectly reliable experiments have proved, is but slightly warmed by the electric current, and the electrodes remain intact or are but slightly attacked by the current. Therefore, in order to determine the dependence in which the electromotive force and the resistance stand relatively to the density of

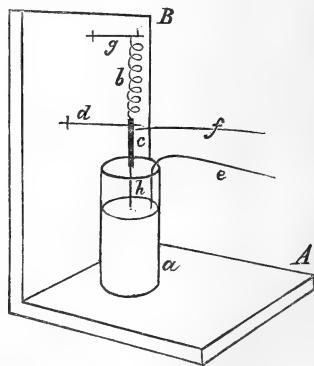
the air, one is forced to rely exclusively on the observations made with more considerable rarefactions of the air.

In order to be able to determine how e and $r_1 l$ vary, each by itself, with the density of the air, it is necessary to proceed with a series of observations in which e and $r_1 l$ are combined in a different manner from that in which they were in the preceding series. For this purpose a Ruhmkorff induction-apparatus was employed in the following manner:—Having found the difficulty there was in obtaining constant deflections when the Foucault interruptor belonging to the apparatus was employed, I made use of only one of the induced currents arising from the opening of the inducing current; and that current was measured by means of a sensitive magnetometer. If the inducing current preserves its intensity without modification, the induced currents do not undergo, at the opening, any modification with respect to quantity, in whatever manner the opening is effected. If the circuit of the induced current is composed of solid and liquid conductors only, the induced currents also produce deflections of equal magnitude on a galvanometer inserted in the circuit. But when the circuit of the induced current is interrupted so that the current is compelled to traverse an extent of air, for the deflections to be equal it is necessary that the opening always take place in the same manner. After some fruitless attempts, this could be done with the aid of a spiral spring arranged in the manner indicated by fig. 2. A B is

a wooden support, on which is placed a glass jar, a , half filled with mercury. On the surface of the mercury is put an ebonite disk perforated at its centre. At g is fixed one extremity of a spiral spring, b , of steel, the other end carrying a small iron cylinder, c , to the opposite extremity of which a thin platinum wire, h , is soldered. When the spring is kept stretched by the lever-arm d , the platinum wire descends

to the mercury through the perforation of the ebonite disk. One of the electrodes, e , of the battery which produces the inducing current dips into the mercury; the other, f , is fixed to the iron cylinder instead of being at g , in order that the spring may not be heated and its

Fig. 2.



elasticity modified by the passage of the current. Now, when the lever is removed, the thin platinum wire is lifted from the mercury, and the current is then interrupted in successive experiments with such uniformity that the currents which pass through the rarefied air give deflections of equal magnitude on the magnetometer.

Experiment 2. The battery that produced the inducing current consisted of six Bunsen elements. The glass tube containing the rarefied air was the same as in experiment 1, with aluminium electrodes placed 5 millim. distant from one another. The passage of the induced current through the rarefied air gave the following deflections:—

Pressures of the gas, in millimetres.	Deflections.
89·8	25·8
79·6	28·0
65·7	29·6
58·6	33·0
39·0	38·8
31·1	41·7
18·9	35·0
8·2	30·0
2·9	19·1
0·58	15·9
0·12	0·0

This series resembles those repeatedly obtained before by other physicists : at a comparatively high pressure of the air the current is weak ; it reaches a maximum at a certain lower pressure, afterwards diminishing again if the rarefaction is increased beyond this. It is from such series of experiments that it has been inferred, *inter alia*, that the electric resistance of air diminishes with the pressure until it arrives at a certain limit, which being passed, the resistance increases again, if the rarefaction continues to increase, and in a vacuum becomes infinite. Here I will only remark, as a preliminary, that the series of deflections of experiment 2 may be regarded on the whole as a sort of inversion of the series of experiment 1. In the latter series the deflections are considerable at the higher pressures of the air, and then diminish till the pressure has fallen to about 30 millim., after which they commence to increase if the increase of rarefaction be continued ; in series 2 the reverse is presented, in that the maximum occurs there at a pressure of about 30 millim.

If E denotes the electromotive force of the induction-apparatus, e that of the spark in the rarefied air, $r_1 l$, as before, the

resistance of the air, and m the resistance of the rest of the closed circuit, the deflections must, according to what precedes, be proportional to $\frac{E - e - r_1 l}{m}$.

If we exclude the rarefied-air space from the circuit, the intensity of the current will be expressed by $\frac{E}{m}$. This intensity, which was measured several times between the observations of series 2, remained (with insignificant variations) constant the whole time. The deflection was too great to be measured by the magnetometer without placing before the latter a bridge to produce a division of the current. It was found by calculation that, without the employment of the bridge, the deflection would have been 1300 scale-divisions. If we subtract from this number the deflections observed in series 2, the remainders will be proportional to $\frac{e + r_1 l}{m}$.

We thus get:—

Pressure of the air.	$\frac{e + r_1 l}{m}$.
89·8	1274·2
79·6	1272·0
65·7	1270·4
58·6	1267·0
39·0	1261·2
31·1	1258·3
18·9	1265·0
8·2	1270·0
2·9	1280·9
0·58	1284·1
0·12	1300

* The deflections obtained at different pressures may be regarded as proceeding from the united action of two different currents, namely:— (1) the current which would be produced by the induction-apparatus in the case in which the rarefied-air space possessed no electromotive force, but merely opposed a resistance to the propagation of the current; as this resistance diminishes as the rarefaction increases, the current in question would gradually increase with the latter; (2) the disjunction-current due to the passage of the former through the rarefied air, and going in a direction opposite to this. If the disjunction-current be subtracted from the current before mentioned, the resultant will arrive at its maximum at the pressure at which the latter current will have its minimum. Now the observations furnished by experiments 1 and 2 show that in the former the minimum is found at nearly the same pressure as the maximum in the latter. Hence we may conclude from this that when in experiment 2 the induction-apparatus was employed in the formation of the current, there was also produced in the rarefied-air space a disjunction-

The deflections, varying only 2 per cent. between the pressures 31.1 and 0.58, may be considered approximately constant. If this invariable deflection be designated by k , and if a is a constant, we can therefore put $e + r_1 l = ak$ for the pressure between these limits. If x be the invariable deflection between the same limits of series 1, and if b is another constant, we can write $e - r_1 l = bx$, from which we get

$$e = \frac{ak + bx}{2} \text{ and } r_1 l = \frac{ak - bx}{2}.$$

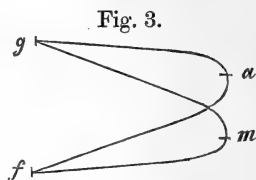
The observations show that x is rapidly augmented with the rarefaction; the following results are obtained from these two series:—*If the density of the air at which the induced current presents its maximum be taken for the starting-point, the electromotive force e increases with the ulterior increase of the rarefaction, while, on the contrary, the electric resistance of the air undergoes continual diminution during the same time.* In the rarefied-air space here employed the resistance of the air diminished by a quantity approximately equal to the increase of the electromotive force when the rarefaction of the air was augmented.

If the distance between the electrodes be made greater than it was in the preceding experiments, the diminution of $r_1 l$, all other conditions remaining equal, will, when the density is reduced, evidently be greater than when l is less. It may therefore easily happen, if l is sufficiently great, that $r_1 l$ undergoes, when the rarefaction is augmented, a diminution exceeding the increase of e , and, consequently, that the sum $e + r_1 l$ continues to diminish with the pressure. In that case the maximum of the induced current, if a maximum be produced, will be found at a pressure below that of the foregoing observations. In Morren's experiments the distance between the electrodes amounted to 240 millim.; and it is very likely that we must attribute to that cause the circumstance that he found the maximum of the current at so low a pressure as 1 millim. or slightly under.

For determining the variations of the sum $e + r_1 l$ at different air-pressures another process was employed, which, however, was not capable of giving so exact a result as the experiments with the induced current. A spark micrometer was used, composed of two brass balls introduced into a wider glass tube, one of which balls could be moved nearer to or further

current similar to that of experiment 1. But it will without difficulty be understood that in experiment 2 the disjunction-current produced in the tube cannot be attributed to any oscillatory discharge; and therefore there is nothing to authorize us to see in a discharge of that nature the cause of the great deflections given by experiment 1.

from the other at pleasure by means of a micrometer-screw, the distance separating them being read on a graduated scale. The spark micrometer and the glass tube with aluminium electrodes used in the previous experiment were connected with the electrophorus in the way indicated by fig. 3; but the distance between the electrodes was now 30 millim., and not 5 as in the foregoing experiments. Two electrodes, connected with one of the receiving combs of the electrophorus, proceed from the point *g* to the metal ball *f* (the same as that denoted by this letter in fig. 1). The glass tube containing the rarefied air is inserted at *a* in one of the electrodes, and the spark micrometer at *m* in the other. When the distance between the balls of the micrometer was great, the discharge passed exclusively through the rarefied-air space; but if they were brought sufficiently nearer, the discharge passed only at *m*, through the spark micrometer. The distance was now regulated so that the successive discharges passed alternately through *a* and through *m*. For this case the sum mentioned, $e + rl$, was regarded as equal in the two tubes; and the distance between the balls, which constituted a measure of that sum, was read on the scale. The following results were obtained:—



Experiment 3.

Pressure of the air. millim	Distance between the balls.
46·5	1·31
25·1	1·31
17·5	1·36
13·0	1·25
6·4	1·14
4·0	1·10
2·3	1·10
0·8	1·15
0·007.	2·33

Experiment 4. In this experiment the aluminium electrodes were replaced by electrodes of platinum, and the distance between them was 30 millim. The following distances between the balls of the micrometer were obtained, according to the modification of the air-pressure in the rarefied-air space:—

Pressure of the air. millim.	Distance between the balls.
117.5	1.30
91.2	1.20
68.3	1.15
21.0	1.15
14.1	1.12
8.0	1.10
6.1	0.95
3.0	1.00
2.38	1.05
0.2	1.40

In these last two series the minimum is found lower down than in series 2—namely, in the first between pressures of 2 and 4 millim., and in the second between 3 and 8 millim. This must probably be attributed to the distance between the electrodes being 30 millim. instead of 5 as in series 2. If, however, the pressure of the minimum be augmented to a higher degree, the value increases more quickly when the distance is greater than when it is less.

Experiment 5. The object of this experiment was to determine whether the electromotive force e is dependent on the quantity of electricity traversing the spark in the rarefied-air space. The electrodes consisted of platinum wires, the distance between which amounted to 30 millim.; pressure of the air 1.8 millim. A rheostat being inserted at w (fig. 1) in the wire that went to the galvanometer, a deflection of 12.6 scale-divisions was obtained; while, if the rheostat was placed at p , in the wire going to the spark, the deflection did not amount to more than 7.1 divisions. When the rheostat was excluded from the circuit, the deflection amounted to 42 divisions. When the rheostat was intercalated at w , of course the quantity of electricity passing through the spark was greater than when the resistance was located at p , although the total resistance of the closed circuit in the two cases was identical. *Consequently the electromotive force increases with the quantity of electricity traversing the spark. These observations show, further, as might have been expected, that the intensity of the current will be diminished if a resistance be inserted in the closed circuit.*

For the case of a constant current (from a sufficiently powerful galvanic battery, for example) traversing the rarefied-air space and producing there an electromotive force e , we must take care not to draw from the foregoing result

the conclusion that e increases or diminishes with the intensity of the current. I have, on the contrary, demonstrated, in a previous investigation on the electromotive force of the voltaic arc, that the above-mentioned force is independent of the intensity of the current to which the arc is due. It was only when the current employed was so weak that it was only just able to produce an arc that the electromotive force appeared to have diminished a minute fraction per cent.* If it is otherwise when the experiments bear upon the electric spark, that must depend on other conditions. When the electricity traverses the rarefied-air space in the form of discontinuous discharges, and the quantity of electricity which passed in the way indicated in the preceding experiment is augmented or diminished, it is possible that not only the duration of the separate sparks, but also the curves indicating the increase of the electricity at the commencement and its decrease at the end of the discharge, undergo modifications. The electromotive force e probably depends on these circumstances, and is not in direct relation with the quantity itself of electricity.

Already in 1868, at the time of my first researches on the electromotive force of the electric spark, I had found that the deflections become smaller if an induction-coil is inserted in the circuit conducting to the galvanometer. In those experiments the spark was formed in a space filled with air. Therefore, of the two induced currents due to the passage of the discharge through the coil, only that which traversed the spark in the same direction as the discharge had power to penetrate the spark; or perhaps, to express myself more correctly, that current traversed the spark with more facility than the induced current which traversed it in the contrary direction. As there might be some interest in ascertaining what would be the results of lowering the pressure of the air in the space in which the spark was produced, the following experiment was made:—

Experiment 6. At w (fig. 1) an induction-coil was inserted, consisting of a copper wire coated with gutta percha and wound in 40 turns of a helix; and then the deflection produced was observed. The coil was now removed, and replaced by a German-silver wire having nearly the same resistance as the induction-coil, and the deflection was observed in the same manner as before. When no spark was formed between c and d , and consequently the entire discharge divided itself between the galvanometer and the bridge, the deflection amounted to 1.5 scale-division. In this way, at different air-pressures the following series was obtained:—

* Pogg. *Ann.* cxxxiii. p. 353; *Phil. Mag.* [4] xxxv.

Pressure of the air. millim.	Deflections.		Difference.
	Without induc- tion-coil.	With induc- tion-coil.	
0.24 . . .	1.75	1.85	+ 0.10
0.99 . . .	27.55	50.05	+ 22.80
1.55 . . .	23.55	37.15	+ 13.60
2.70 . . .	23.0	17.65	— 5.35
3.20 . . .	21.0	9.80	— 11.20
4.50 . . .	4.55	0.95	— 3.60
6.00 . . .	7.06	2.25	— 4.80
8.65 . . .	5.95	1.85	— 4.10

There are some important circumstances to be noticed in this series, to which another series, not reproduced here, was perfectly conformable. Without the induction-coil the deflections increase in magnitude from a pressure of 4.50 to 0.99 millim. The value of $e - r_1 l$ undergoes therefore continuous augmentation from the first limit to the second. Now, between these limits the expression $e + r_1 l$ is, according to the series 3 and 4, nearly constant. From this it follows that e increases continuously between the limits in question, while $r_1 l$ undergoes continuous diminution. If the pressure descends to 0.24 millim., the sum $e + r_1 l$ increases notably, according to series 3 and 4. Now, when we increase the resistance of the circuit $kcdh$ (fig. 1), the quantity of electricity that traverses the rarefied-air space is diminished, whence it follows, according to series 5, that the deflections are diminished also. It is here that we must seek the reason of the fact that at 0.24 millim. pressure of air the deflection amounts to only 1.75 scale-division. This series therefore gives the same result as experiment 1.

On the other hand, the observations taken when an induction-coil was inserted in the circuit at w or at p gave a partially unexpected result. The deflections diminished for a pressure between the limits 8.65 and 2.70 millim. Of the two induction-currents, going in opposite directions, due to the passage of the discharge-current through the coil, that which traverses the spark in the inverse direction of the disjunction-current, and consequently also traverses the spirals of the galvanometer in a direction opposed to that current, has the property of asserting itself principally or exclusively. The spark, then, acts as a sort of valve; it lets one of the induction-currents pass, but not the other, or at least opposes a greater obstacle to the passage of one of the currents than to the passage of the other. This is perfectly conformable to what I

have found to be the case when the spark is formed in a space filled with air*; while the opposite was presented at pressures below 2·7 millim.: the deflections then became greater; that is to say, the stronger current was that which traversed the spark in the same direction as the disjunction-current. This remarkable property of the spark in the rarefied-air space would perhaps deserve a more special study by means of a particular investigation.

When the discharge-current passes through the galvanometer-coil, of course two induction-currents in opposite directions are produced in that coil also. For preventing the action of these currents upon the magnetized needle it suffices to insert a wire of sufficient resistance, as a bridge, before the galvanometer-coil; in fact, as the resistance of this bridge can be made much less than the total resistance of the rarefied-air space, the induced currents, which are of equal intensity but opposite in direction, pass almost exclusively through the bridge, and consequently cut off their action upon the magnetized needle. I convinced myself, by the following simple procedure, that their effect upon the needle became really insensible:—An induction-coil, of approximately the same quality and size as that of the galvanometer, was placed at *w*. The deflections were then seen to be considerably diminished if the pressure was greater, but to be increased when the pressure was less. When, on the contrary, a wire of suitable resistance was placed as a bridge before the induction-coil, the deflections became perfectly equal, whether the induction-coil was or was not inserted in the circuit. As in all the experiments the galvanometer-coil was always provided with a bridge offering a suitable resistance, the induced current exercised no sensible influence upon the results obtained.

The electric current due to the passage of the discharge through the rarefied-air space has been expressed by $\frac{e - r_1 l}{m}$, in conformity with the reasons given in the preceding pages. In regard to this, it should be remarked that *m* (that is to say, the resistance of the part of the circuit composed of wires) is infinitely small in comparison with the resistance $r_1 l$ of the rarefied air. The following experiment has proved that the current in question cannot be calculated from the usual formula of Ohm $\frac{e}{m + r_1 l}$, or since *m* can be neglected in comparison with $r_1 l$, by the formula $\frac{e}{r_1 l}$.

* *Öfversigt af Kongl. Vetensk.-Akad. Förhandl.* 1868; *Pogg. Ann.* cxxxvi.; *Ann. de Chim. et de Phys.* [4] xvii.; *Phil. Mag.* [4] xxxviii.

Experiment 7. Two perfectly similar glass tubes, provided at their extremities with electrodes consisting of platinum wires, were connected with the mercury air-pump so that there might be equal pressure of air in both. The electrodes were covered with glass tubes to the distance of 1 millim. from their extremities. The distance between the electrodes amounted to 5 millim. in one of the tubes, and six times that sum, or 30 millim., in the other. Although the electrodes had been rendered as equal as possible, from some unknown cause the electromotive force of the tube in which the distance between the electrodes amounted to 30 millim. was greater than in the other. With these two tubes the following deflections, at different pressures of the air, were obtained:—

Pressure of the air. millim.	Deflections.	
	Distance 30 millim.	Distance 5 millim.
0·23	5·6	2·6
0·73	17·8	14·5
1·15	16·9	16·1
2·13	5·5	9·1
5·40	3·2	5·8
8·13	1·6	2·2

Thus, at the lower pressures from 0·23 to 1·15 millim. the current in the tube in which the distance between the electrodes amounted to 30 millim. was stronger than that of the other tube; while the opposite took place from the last-mentioned pressure to that of 8·13 millim. If we call the electromotive force of the tube with 5 millim. distance between the electrodes e , and that of the other tube e_1 , and if r_1l denotes the resistance of the gas in the first tube, and consequently $6r_1l$ the force of resistance of the second, we get, according to

Ohm's law, $\frac{e}{r_1l}$ and $\frac{e_1}{6r_1l}$. The observations above given show that at a pressure of 0·73 millim., for example, $\frac{e_1}{6r_1l} > \frac{e}{r_1l}$. If

we pass thence to a greater pressure, as, for instance, 5·4 millim., e_1 and e will be modified in the same proportion, and consequently their amount at that pressure will be expressed by pe_1 and pe , in which case, from what precedes, p is less than unity. The resistances change in the same manner into $n6r_1l$ and nr_1l , where n denotes a number higher than unity. We

get, therefore, at the higher pressure, $\frac{p_1}{n6r_1l}$ and $\frac{pe}{nr_1l}$; but, whatever numbers p and n may represent, the former fraction

must, according to what has been said above, be greater than the latter. On the other hand, the observations show that the deflection at a pressure of 5.4 millim., for example, is smaller when the electrodes are at a greater distance from one another than when they are closer. Ohm's law therefore, in its original form, is not applicable to the case in which the current passes through a gaseous body.

When, on the contrary, the current-intensities are proportional to $e - r_1 l$ and $e_1 - 6r_1 l$, there is no contradiction in the circumstance that at the low pressure $e_1 - 6r_1 l > e - r_1 l$ while at the same time, at the higher pressure, $pe_1 - n6r_1 l < pe - nr_1 l$.

Experiment 8. In this experiment on the passage of electricity through rarefied air, one of the electrodes consisted of an aluminium wire, and the other of a platinum wire of approximately the same thickness. The discharge passed alternately from the platinum to the aluminium, and from the latter electrode to the former. In the circuit, at w , a rheostat with a resistance of 21.5 ohms was inserted. In this way the following results were obtained:—

Pressure of the air. millim.	Deflection when the discharge went	
	from aluminium to platinum.	from platinum to aluminium.
2.2	11.65	6.85
3.4	14.35	7.22
3.8	15.05	7.05
6.5	11.90	6.40
24.1	5.10	3.30
35.7	3.55	2.85

If the electricity did not pass through the rarefied-air space, a deflection of 1.9 scale-division was obtained when the discharge from the electrophorus passed through the galvanometer circuit.

The disjunction-current was therefore always less when the discharge passed from platinum to aluminium than when it pursued the contrary direction. Consequently aluminium as the negative pole of the discharge gives a weaker disjunction-current than a platinum negative pole. As, with such slight pressures of air as the preceding, no particles of metal are conveyed in the gaseous state from one pole to the other, the air between the poles is not mixed with foreign particles during the discharge; and hence its resistance $r_1 l$ is not dependent on whether the discharge passes from aluminium to platinum or whether it takes the contrary direction. The cause of the difference between the amount of current in the

two cases must therefore depend on the modification of the electromotive force e . The experiments made show that this force is dependent on the chemical nature of the poles, and also that in this respect the two poles play essentially different parts. These facts are in accordance with the observations of Hittorf, according to which an induced current traversing a space of rarefied air meets with a notable resistance at the negative pole, but less if that pole consists of aluminium than if it is of platinum*. Now, as we have seen from what precedes, the obstacle encountered by the current at the negative pole does not consist of an electric resistance in the usual acceptation of the term, but of an electromotive force tending to send a current contrary in direction to the principal current.

It has been observed that the negative pole is strongly heated in a space of rarefied air, and that, *cæteris paribus*, this heating is augmented with the rarefaction of the air. I have, in a previous memoir, proved that, when a current traverses an electromotor in an opposite direction to the current which the electromotor tends to produce, there results from it an evolution of heat proportional to the electromotive force multiplied by the intensity of the current†. Now, since (according to what precedes) the electromotive force e in the spark is notably augmented with the rarefaction, it follows that the heat evolved at the negative pole must be increased when the rarefaction is increased, which accords with the results of experiment. Here it should be remarked that in the voltaic arc it is the positive pole that is most strongly heated, from which we may be authorized to conclude that the electromotive force shown by irrefutable proofs to exist in the voltaic arc has its seat at the positive pole. This force is not thermoelectric, and it is not produced, as M. Le Roux‡ assumes, by one of the poles being more highly heated than the other; on the contrary, it is the electromotive force that is the cause of the heating. It has been proved that the force continues to exist, even if the negative pole of the voltaic arc be heated, by a Bunsen burner or in any other way, so as to become hotter than the positive pole§.

The researches of which I have just given a brief summary confirm therefore, in all respects, the results to which I had

* Pogg. Ann. cxxxvi. p. 25 (1869).

† Kongl. Svenska Vetenskaps-Akademiens Handlingar, xiv. (1876); Pogg. Ann. clix.; Phil. Mag. [5] iii.

‡ Comptes Rendus, xcii. p. 710 (1881).

§ Öfversigt af Kongl. Vetensk.-Akad. Förhandl. 1868, p. 12; Pogg. Ann. cxxxiv.; Phil. Mag. [4] xxxvi.

already been led by my examination of the observations of other physicists on the passage of electricity through rarefied gases. The maximum attained by the current-intensity at a certain pressure of the air when a current traverses a space of rarefied air is not due in any way, as it has been generally assumed to be, to the resistance $r_1 l$ of the air having its minimum at that pressure, and afterwards increasing in amount with the increase of rarefaction, but really to this—that the sum $e + r_1 l$ then possesses its minimum value. With the continuation of the rarefaction the resistance $r_1 l$ continues to diminish, while e incessantly increases. Consequently the circumstance mentioned above, namely that the induced current possesses its maximum value at a certain pressure of air, gives no support to the allegation that in highly rarefied air or in a vacuum the resistance is sufficiently great to prevent the current passing. Here it is not the resistance of the gas, but the electromotive force e_1 increasing with the rarefaction and connected with the electrodes, that presents an obstacle to the passage of the current. Every thing is in favour of the hypothesis that vacuum opposes a very feeble resistance to the propagation of electricity. One can therefore, without the employment of electrodes, by induction at a distance, or by friction at the surface of a tube in which the air is sufficiently rarefied to render the passage of a strong induction-current between the electrodes impossible, easily excite in that tube an electric motion sufficiently considerable to produce a sensible development of light. Now this would be impossible if highly rarefied gas or a vacuum were an insulator.

II. *On a Wedge-and-Diaphragm Photometer.*

By ROBERT SABINE*.

THE photometer described by me in a paper contributed to the last Meeting of the British Association at Southampton† was based upon the weakening of the light to a constant value by the interposition of sheets of some absorbing material as far as could be, and finally obtaining a balance by varying the distance of the photometer from the light to be measured.

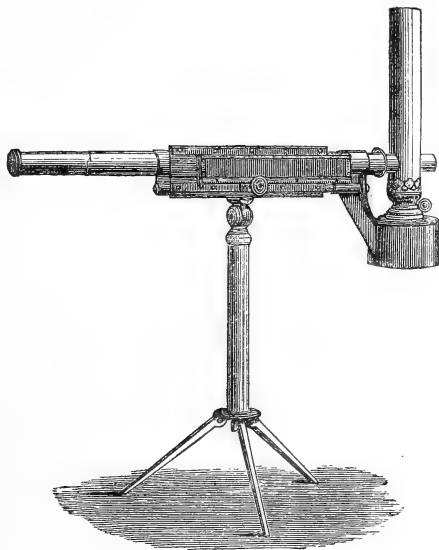
I find it, however, frequently in practice more convenient to keep the photometer at a constant distance from the source of light, and to effect a balance by the gradual increase of the thickness of the absorbing material only, using at the same

* Communicated by the Author.

† Electrical Review, vol. xi. p. 197.

time different illuminations of the field of comparison for extending the range of the instrument.

The photometer I have constructed is shown in perspective in the figure. It consists of a brass tube mounted upon a



stand, and having an eyepiece at one end and a paraffin-lamp at the other. The light of the paraffin-flame, entering at the end of the tube, falls upon a disk of opal glass placed near the middle of the tube, and keeps it constantly illuminated. In the middle, the tube is cut away and a collar fitted over it, which can be turned into any required position. This collar has at one side of it a slit containing a strip of opal glass, opposite which it has fixed to it a frame carrying a wedge of neutral-tinted shade glass, corrected by being combined with a reversed wedge of white glass, with which it is mounted in a slide with rack-and-pinion adjustment. The thicker end of the wedge absorbs about eight times as much light as the thinner end; that is to say, the light from eight candles, after passing through the thicker end, has the same illuminating effect as the light of one candle after passing through the thinner end.

When the light to be measured, which is placed on the right-hand side of the photometer, is above or below the horizontal, the wedge is turned, with the collar which carries it, so that the rays from the light, whatever may be its position, fall

normally upon the face of the wedge. The light then passes through and is weakened by the wedge, then through the strip of opal glass, through the slit behind it into the interior. Here it is incident upon a small mirror made of a strip of silvered microscope covering-glass, placed at an angle of 45° to the axis of the tube, and is thence reflected to the eye of the observer. The other end of the photometer, close to the bright part of the paraffin-flame, is furnished with a cap containing a circular diaphragm disk with three different apertures, either of which can be inserted by simply turning the disk. The little mirror is supported from the face of the constantly illuminated opal pane in such a way that the support is hidden from the observer by the mirror. The object of this arrangement is to ensure the observed surfaces whose illuminations are to be compared being in juxtaposition without any apparent division-line or separation. It is well known* that when the two surfaces to be compared are at a perceptible distance apart, as in Rumford's photometer, no certainty of accurate measurement is possible. The method of supporting a small mirror in the centre of the field of comparison in the way described is very satisfactory. The glass of the mirror being extremely thin, I find with a moderate illumination, and when the light reflected by the mirror is of the same colour as that of the field, the mirror sometimes entirely vanishes from sight at the moment of balance.

The distance at which the light to be observed is placed from the photometer varies, of course, according to convenience; but where it is optional, I prefer for all lights under 1000 candles a constant distance of 1 metre. Stronger lights become inconvenient to the observer if placed so near; and in that case I increase the distance accordingly, while of course at the same time increasing the range of the photometer.

The degree of illumination of the field of comparison is adjustable by means of the diaphragms. The smallest diaphragm illuminates the field so that the light of a single candle can be conveniently balanced. The next diaphragm exposes an area about eight times as large of the bright portion of the paraffin-flame, and the largest diaphragm about sixty-four times; that is to say, it has about eight times the diameter of the smallest diaphragm. The resulting degrees of illumination of the field can be thus calculated as well as compared by actual observation. Let the illumination of the field with these three diaphragms be respectively λ_1 , λ_2 , λ_3 . Of the light which falls upon the outer face of the wedge a

* Herschel, 'On Light,' p. 29; Helmholtz, *Physiologische Optik*, p. 329.

fraction is, of course, reflected and lost; the remaining fraction (s) enters the wedge. Let l be the illuminating-power of the light (say a standard candle) at a unit distance from it, in the direction of the photometer, n the thickness of the wedge at the point interposed, m its coefficient of transmission, and d the distance of the light from the opal pane in the slit; then the illuminating-power of the light which reaches the opal pane will be

$$\frac{slm^n}{d^2}.$$

This light falls upon the surface of the opal glass; a fraction enters it, part of which is absorbed, the remainder passing to the mirror, a further part being absorbed in the reflection to the eye of the observer. After the further diminution up to this point the fraction of the light which leaves the opal pane and which actually reaches the eye is s_1 , the apparent illumination of the mirror with the smallest diaphragm being

$$\lambda_1 = \frac{ss_1lm^n}{d^2}.$$

Similarly, if we now remove the candle and substitute some other light whose illuminating-power at unit distance is L and its actual distance D , the wedge will have to be readjusted to n_1 ; and if beyond the range of the wedge, the field will have to be adjusted by the diaphragm being increased (say to the illumination of the field λ_2); then

$$\lambda_2 = \frac{ss_1Lm^{n_1}}{D^2};$$

the relation of the illuminating-powers of the two lights will be

$$\frac{L}{l} = \left(\frac{D}{d}\right)^2 \frac{\lambda_2}{\lambda_1} m^{n-n_1}.$$

The value of $\frac{\lambda_2}{\lambda_1}$ is ascertained experimentally by obtaining a balance with some constant light at different distances, as well as by measuring the openings of the diaphragms. The values d and m^n for the standard candle are constants.

$$\text{Let } \frac{m^n}{d^2} = \alpha, \text{ then } \frac{L}{l} = \frac{\lambda_2 \alpha}{\lambda_1 m^{n_1}} D^2.$$

The wedge is provided with a scale divided into millimetres, by which the position is observed. A table is constructed

giving the values of $\frac{\alpha}{m^{n_1}}$, which is determined for each position of the wedge by obtaining a balance with a constant light at different distances, and for each of the diaphragms controlling the illumination of the comparison-field. In using the photometer in practice, it is therefore only necessary to obtain a balance and to read off the indication of the scale. A reference to the table then gives the value which, multiplied into the square of the distance in metres, gives the candle-power of the measured light, or, if the distance be exactly 1 metre, gives the candle-power direct.

It happens sometimes that the degree of illumination to be ascertained is independent of distance, as in the case of daylight; in this case the diaphragm and wedge are adjusted until a balance is obtained. The value given by the table then represents the number of standard candles which at 1 metre distance would illuminate an object to an equal degree.

In measuring the illuminating-powers of lights the colours of the lights have sometimes to be taken into account. The way I prefer to do this is to assume the ingredient of orange light contained in daylight as unit for comparing the relative ingredient of orange light contained in the artificial light which is measured. By orange light I mean that ingredient of light which passes freely through orange glass, and which in fact embraces nearly all that portion of the visible spectrum between the lines B and F—that is to say, the red, orange, yellow, and a small part of the green rays; whilst with a moderate illumination, such as is used in photometric comparisons, all the more refrangible and less luminous rays from the line F upwards are quenched.

For convenience of observing, the photometer is furnished with a small disk centred inside the eyepiece and containing small panes of white and orange glass*, either of which can be instantly interposed when required.

A balance of illuminating effect is first estimated with the white pane irrespective of colour; then the disk is turned so as to interpose the orange pane, and the wedge readjusted accordingly. If with the orange glass the reading is $\left\{ \begin{array}{l} \text{higher} \\ \text{lower} \end{array} \right\}$ than with the white (that is, if $\left\{ \begin{array}{l} \text{less} \\ \text{more} \end{array} \right\}$ light has to be admitted through the wedge in order to maintain the balance),

* The disk contains also red and green glasses, and may be furnished with any other colours for which the observer has a fancy.

it follows that the observed light contains proportionally
 $\left\{ \begin{array}{l} \text{more} \\ \text{less} \end{array} \right\}$ orange than the paraffin-flame.

Thus with the white-glass pane daylight in the laboratory was balanced by a reading which the table indicated to be equal to 55 candles at 1 metre. Observed through the orange glass, however, the reading indicated the orange ingredients in the daylight to be equal to 42 candles; that is to say, with the paraffin-flame the light which passed through orange

glass was $\frac{55}{42} = 1.3$ times the corresponding ingredient of the

same coloured light in daylight. The light of an incandescent lamp through white glass was 28 candles, while through the orange glass it was 26 candles; therefore the

incandescent lamp had $\frac{26}{28} = 0.94$ time the corresponding

colour in the paraffin-flame; and as the paraffin-flame had 1.3 as much as daylight, the incandescent lamp had $1.3 \times 0.94 = 1.2$ as much as daylight. In other words, if we had a similar quantity of light obtained from daylight and from incandescent lamps, we should have in the latter about 20 per cent. more orange-tinted light.

This colour-comparison is of course only very rough; but it enables the excess of orange light in incandescent lamps, and its occasional deficiency in some arc lamps, to be approximately compared.

I have been led to employ in practice a selected portion of a paraffin-flame in preference to a standard candle, partly in consequence of the fact that the paraffin-flame can be enclosed in a light-tight lantern, but chiefly on account of its superior steadiness and constancy. As an instance, I have obtained a balance between selected portions of two paraffin-flames, which balance has been maintained for hours at a time without the slightest variation. On the other hand, when a standard candle was compared with the bright part of a paraffin-flame, the photometer had to be readjusted continually, showing several-per-cent. variations in the burning of the candle. I therefore prefer to take a long series of readings once for all in determining the value of the light admitted to the comparison-field of the photometer by each diaphragm, to assume means of these as constants of reduction to the nominal standard, and to depend upon the constancy and steadiness of the lights given through the diaphragms by the paraffin-flame afterwards. After working practically with this system for some time, my opinion is that a selected area of the bright

part of a paraffin-flame would be a much better unit of light than the present standard candle, particularly as distances would be measured from the diaphragm and not from the flame, by which error due to the curvature of the flame would disappear.

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III. On Lockyer's Theory of Dissociation.

By HERMANN W. VOGEL*.

IN February 1880 I took an opportunity to criticise, on the ground of my observations of the spectrum of chemically pure hydrogen, Lockyer's view that calcium is dissociated at a very high temperature†. Lockyer takes for his starting-point, *inter alia*, that in the spectra of the so-called white stars, photographed by Huggins, only the first of the two calcium-lines H' and H'' is present, and accordingly advances the theory that calcium at a high temperature separates into two substances X and Y, of which the first gives the line H' , the other the line H'' , and that, in the stars alluded to, only the first exists. I showed, on the contrary, that hydrogen possesses, besides the four well-known readily seen lines, another, singular line with extremely intense photographic action, which nearly coincides with Fraunhofer's line H' , and that we are so much the more entitled to hold that the supposed calcium-line observed by Huggins is the fifth hydrogen-line, as the known lines of hydrogen are especially well developed in the spectra of those stars, and also the ultra-violet star-lines observed by Huggins coincide with the ultra-violet hydrogen-lines photographically fixed by me‡.

Lockyer, however, has not given up his notion about dissociation, but has sought in spectroscopy for new evidence in favour of it.

He remarks that, *inter alia*, in the spectrum of the sun-spots certain iron-lines appear widened, others not, and, further, that many of them, as $\lambda 4918$ and $\lambda 4919.7$, do not occur in the spectrum of the protuberances (which show other lines of iron), but certainly do in the spectrum of the spots, while in these, again, iron-lines which the former contain are, under some circumstances, wanting; and he then continues,

* Translated from the *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin*, Nov. 2, 1882, pp. 905-907.

† Proc. Roy. Soc. xxviii. p. 157.

‡ *Monatsb. d. Berliner Akad. d. Wissensch.* 1880, p. 192.

“Consequently there is no iron in the sun, but only its elements”*.

Liveing and Dewar† have already controverted this reasoning, by showing that certain spectral lines of a substance, *e.g.* $\lambda 5210$ of magnesium and various calcium-lines, become visible only when certain foreign substances are present—in the case in question, hydrogen on the one hand, and iron on the other,—that consequently the non-appearance of certain iron-lines in the spectra of the spots or protuberances ought not to be attributed to a dissociation, but to the absence of foreign substances which produce the strong coming-out of those lines.

Lockyer, however, now relies upon another fact, which is not accounted for by the experiments of Liveing and Dewar, and which at any rate seems to give a firmer support to his dissociation theory than those above mentioned. He says:—“The last series of observations refer to the degree of motion of the vapours in the sun-spots, which, as is known, is indicated by changes in the refrangibility of the lines. If all the lines of iron in a spot were produced by the vapour of iron moving with a velocity of 40 kilometres in a second, this velocity would be indicated by an alteration of the refrangibility of *all* the lines. But we find that this is *not* the case. We not merely ascertain different motions indicated by different lines, but have observed in the degree of motion the same changes as in the breadth of the lines. This fact can be easily explained if we assume dissociation; and *I know no simpler way of explaining it*”‡.

Lockyer mentions, as an instance, that in the spots on the 24th December 1880, and the 1st and 6th January 1881, a certain number of iron-lines appeared curved, while others remained straight.

Now I believe I can explain these facts from numerous observations in absorption-spectrum analysis, without needing to have recourse to the hypothesis of dissociation.

It is known that the position of the absorption-bands of a substance essentially depends on the dispersion of the medium in which it is dissolved or incorporated. It is often noticed that in strongly dispersing media the absorption-bands of a substance are displaced towards the red§. Now the remark-

* *Comptes Rendus*, xcii. p. 366.

† *Proc. Roy. Soc.* xxx. p. 93; Wiedemann's *Beiblätter*, iv. p. 366.

‡ I have here followed the rendering of Lockyer's memoir given in the *Naturforscher* for June 4, 1881, in order to exclude any appearance of individual colouring of the translation.

§ Kundt, Pogg. *Ann.* Jubelband, p. 620.

able case not unfrequently occurs, that with the increase of the dispersion of the solvent certain absorption-bands are displaced, and others not. Hagenbach observed, for instance, that bands I., III., and IV. of chlorophyll in an alcoholic solution are situated more towards the red than those of chlorophyll in an etheric solution, while band II. shows the same position in both solutions; and I have observed similar instances in connexion with protosalts of uranium* and combinations of cobalt†.

Now Kundt has already (*loc. cit.*) remarked that the same rules hold for the absorption-spectra of gases as for those of liquids. It is true that he adds, "Only it is questionable whether, when hyponitrous acid gas, for example, is mixed with various other transparent gases, the displacements of the absorption-bands are so considerable as to be perceptible;" this doubt, however, does not concern the rule above-mentioned, but only the possibility of testing it by experiment‡. Hence it is an admissible assumption that, in like manner as in liquids, so also in gases, media mixed with them act upon the position of the absorption-bands, and that, in these as in those, displacements of single bands may occur while the position of others remains unchanged.

If, therefore, in the sun-spots some lines undergo displacement and others in the same place do not, motion is not the cause, but the mixture of a foreign strongly dispersing gas, which acts upon the lines displaced and not upon the others. From this it further follows that curving of absorption-lines of the sun-spots must not by any means always be interpreted as motion of the absorbing gases in the direction of the line of observation, but only when *all* the lines of a substance partake of the curvature.

That also bright lines of luminous gases under similar circumstances may suffer displacement "through mixture of another, not luminous, or a vapour giving a continuous spectrum," Kundt has already pointed out (*loc. cit.* p. 620).

Berlin, October 1882.

* Vogel, *Pract. Spectralanalyse*, Nördlingen bei Beck, p. 248.

† *Monatsb. der Akad. der Wissensch. Berlin*, May 20, 1878.

‡ Kundt previously doubted also the possibility of demonstrating an anomalous dispersion in gases and incandescent vapours; but he has recently succeeded in demonstrating it in the vapour of sodium (*Wiedemann's Annalen*, x. p. 321).

IV. *The Electrical Resistance of Selenium Cells.*

By SHELFORD BIDWELL, M.A., LL.B.*

IN June 1881 a paper was read before the Physical Society by Dr. Moser, on "the Microphonic Action of Selenium Cells." In this paper a very ingenious attempt was made to show that the effect of light in reducing the electrical resistance of selenium might be accounted for on perfectly well understood principles, without assuming the existence in the case of this substance of some law or property *sui generis* and hitherto unobserved.

Dr. Moser's theory is shortly as follows:—There is always imperfect contact between the metallic electrodes and the selenium which together constitute a so-called "selenium cell." Selenium reflects the invisible portions of the spectrum, absorbing principally the visible or illuminating rays: the vibrations thus taken up assume the form of heat; and the temperature of the selenium cell is thereby raised†. In consequence of this rise of temperature the selenium expands; it is accordingly pressed into closer contact with the electrodes, and, as in the case of the microphone, the resistance of the system is proportionately diminished. When the cell is screened from the light, the absorbed heat is quickly radiated away; the selenium contracts to its former volume, and the original degree of resistance is restored. Thus, according to Dr. Moser's paper, the whole mystery is easily and completely explained.

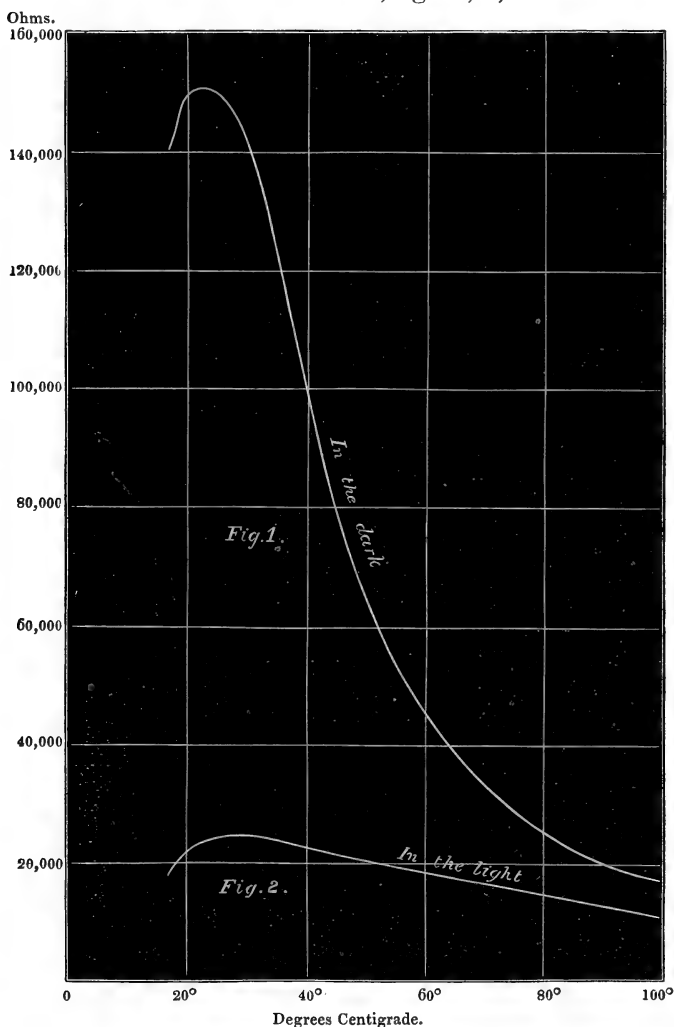
This theory can evidently be submitted to a very simple and conclusive test. If it is true that the observed effects are due merely to a rise of temperature, then it is clearly immaterial whether such rise of temperature is brought about by the heating action of light or by the direct application of heat in the ordinary way. Instead of exposing a selenium cell to the light, let it be enclosed in a dark box and warmed over a gas-burner; then, if the theory be correct, the resistance of the cell should at once begin to fall. This, however, is not found to be the case. I have in my possession a number of selenium cells the resistance of which is immediately diminished by the smallest accession of light; but in the case of all of them (except one, of which I shall say more presently) the immediate effect of the direct application of heat is not a fall, but a rise in the resistance. When the temperature of the cell reaches a point which is in general a few degrees

* Communicated by the Physical Society, having been read at the Meeting on November 25, 1882.

† "*Selenium*," Dr. Moser says, "*is heated by light*."

higher than the average temperature of the air a maximum resistance is attained; and if the heating is continued, the resistance begins to decrease.

I gave a short account of this phenomenon in the 'Philosophical Magazine' of April 1881. Since this was published, I have made further and very careful experiments, the results of which are shown in the curves, figs. 1, 2, and 3.



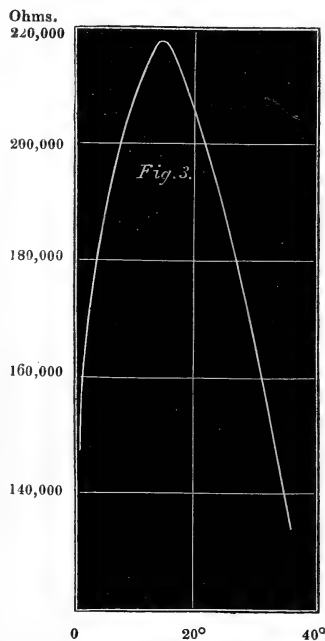
A selenium cell was placed in an air-bath in absolute dark-

ness, the bulb of a thermometer being very near its surface. The temperature of the air was 17°C ., and the resistance of the cell at the beginning of the experiment was 140,000 ohms. The bath was then very slowly heated, and the resistance measured at every degree. At first the rise was very rapid (see fig. 1); then more gradual until the temperature reached 23° , when the maximum resistance of 150,000 ohms was attained. With continued heating the resistance fell, slowly at first, then more rapidly, then again slowly (as shown by the curve), the final measurement at 100° being only 16,000 ohms*.

The same cell was afterwards submitted to the combined action of heat and light. A glass beaker was fitted with a wooden cover, to which the selenium cell was attached so as to hang perpendicularly inside the beaker; the beaker was placed in a sand-bath which was heated by a Bunsen burner, and the cell was illuminated by a powerful paraffin-lamp at a distance of 30 centimetres.

At 18° its resistance was only 19,000 ohms (see fig. 2). As in the former case, the first application of heat was accompanied by a rise, though smaller and more gradual than before, the maximum of 24,000 ohms occurring at about 29° . The fall which followed was also very gradual, the resistance at 100° being 10,000 ohms, or only 14,000 less than the maximum, as against a difference of 140,000 in the former experiment.

In these experiments it might possibly be suspected that the initial small rise of resistance is due to some accidental disturbing cause, and does not point to any essential characteristic of selenium, or rather perhaps of selenium cells. The following experiment seems, however, to settle the point conclusively. One of my selenium cells (the exceptional one



* When the cell was removed from the air-bath, its resistance in the dark in air at 18° was found to have increased to 90,000 ohms.

above referred to) did not at ordinary temperatures exhibit this peculiarity. When heated, its resistance at once went down without any preliminary rise. This cell was placed in air at a temperature of 0° , and after remaining for half an hour its resistance was found to be 147,000 ohms. The temperature was then slowly raised; and, as I expected, the resistance at first went up, attaining a maximum of 219,000 ohms at 13° , after which it went down to 134,000 ohms at 36° , when the experiment was stopped. The curve fig. 2, which is on the same scale as the others, shows the results in a very striking manner, altogether excluding the possibility of accidental disturbance. This particular cell differed from others only in the fact that it acquired its maximum resistance at a temperature slightly below instead of slightly above the average temperature of the air.

The supposition that light produces its effect by heating is further negatived by the fact, that a comparatively high degree of temperature is required to bring down the resistance of the cell to the point to which it is instantly reduced by exposure to a strong light. When a selenium cell is for a moment exposed to sunlight, it does not become perceptibly warm to the touch; but the amount of dark heat necessary to effect the same reduction in its resistance as is caused by a moment's sunshine would certainly render it too hot to handle.

Again, those who have experimented with the photophone know well that the best results are obtained only when precautions are taken to exclude those rays which are especially instrumental in producing heat, as by filtering the beam of light through a solution of alum. Dark radiation does indeed *per se* diminish the resistance of selenium; but the diminution due to dark radiation is to some extent masked by the rise of temperature which accompanies it, and which generally tends to produce the opposite effect.

To me it seems clear that the electrical effects of radiation are, in this case at least, no more due to the intermediate action of temperature than are the chemical effects which radiation sometimes produces, as in the various photographic processes. All such effects are no doubt ultimately of a mechanical nature; but while increased temperature may result from vibrations the periodicity of which vary between very wide limits, the other effects arise only when there is some more or less definite relation between the period of the æther-waves and the molecular constitution of the substance upon which they act.

In its peculiar sensitiveness to the visible part of the spectrum selenium seems, so far as our present knowledge goes, to stand almost, if not quite, alone*.

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Wandsworth, S.W.

V. *On Central Forces and the Conservation of Energy.* By
WALTER R. BROWNE, M.A., M. Inst. C.E., late Fellow of
Trinity College, Cambridge†.

IT is well known that the ordinary proof of the principle known as the Conservation of Energy assumes the forces acting to be Central Forces‡; but the intimate connexion existing between these two facts—the existence of Central Forces and the Conservation of Energy—has not, so far as I am aware, been thoroughly examined. I shall here attempt to show that the two necessarily imply each other; so that not only is the Conservation of Energy true if the system is a system of central forces, but the Conservation of Energy is not true if the system is any thing but a system of central forces.

For the sake of simplicity I will confine myself to the case of two particles, and suppose them so far apart, in proportion to their dimensions, that each may be treated as if concentrated at its centre of gravity. Let the particles be A and B, and consider the motion of B with reference to A as fixed. Suppose B to be moving away from A, and to be acted upon by a moving force due to the action of A. Let it move from a distance a to a distance $a+b$, and let F be the resolved part of the moving force in the line AB. Then the energy exerted by B during this motion in overcoming the attraction of A is represented by

$$\int_a^{a+b} F dx.$$

Let v_1 be B's initial velocity, m its mass. Then at the end of the motion v_1 will be reduced to v , where v is given by the

* So far as regards Dr. Moser's application of his theory to the carbon photophone of Messrs Bell and Tainter, I entirely agree with him; my own experiments showing conclusively that the effects are due to heat only. But the best carbon cells are vastly inferior in their action to those of selenium.

† Communicated by the Physical Society, having been read at the Meeting on November 11, 1882.

‡ This is recognized explicitly by Clausius, 'Mechanical Theory of Heat,' p. 16.

equation

$$\frac{m}{2}(v_1^2 - v^2) = \int_a^{a+b} F dx.$$

Let us suppose b to be such that $v=0$, so that

$$\frac{m}{2}v_1^2 = \int_a^{a+b} F dx. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Then, when B arrives at distance $a+b$, its velocity, and therefore its kinetic energy or *vis viva*, will be reduced to zero. There is therefore a loss of energy, so far as B is concerned. But now let us suppose that B is left free to return towards A, and that it passes back again over the space b . Then, if F continues to act, A will exert during the motion an amount of energy on B, or will do an amount of work upon B, which will be represented by

$$\int_{a+b}^a F dx;$$

and when B has reached the distance a , it will have gained a velocity V , given by the equation

$$\frac{m}{2}V^2 = \int_{a+b}^a F dx. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Now if $V = -v_1$, then $V^2 = v_1^2$: hence we shall have the two particles in the same position as at first, and the kinetic energy of B will be the same as at first. Therefore there will have been no loss or gain of energy on the whole; and the energy is then said to have been *conserved* during the motion. At the time when B's velocity is zero, the energy of the system is represented by the potential energy of A—that is, the

power A has of subsequently doing the work $\int_{a+b}^a F dx$ upon B.

At other times during the motion, the energy of the system is partly potential energy of A, partly kinetic energy of B.

We thus see that it is essential to the Conservation of Energy that V^2 should $= v_1^2$. But by equations (1) and (2) this is equivalent to the equation

$$\int_a^{a+b} F dx = \int_{a+b}^a F dx; \quad . \quad . \quad . \quad . \quad . \quad (3)$$

these two expressions representing the two amounts of energy exerted, as described above.

It is therefore essential for the conservation of energy that

F should be a function of a form such that equation (3) may hold. We have now to inquire what this form must be, or, in other words, within what limits F may be allowed to vary so that the equation (3) may still hold.

Now we have not supposed the constitution of A and B , or their relations to each other, to vary in any way except in regard to space and in regard to time; and we have every reason to believe that these are the only variations which take place in the ultimate molecules of matter. Hence we need only consider variations with regard to space and time.

Now if F be any function of time, then, since some time must have elapsed between the exertion of the two amounts of energy represented by the two sides of equation (3), it follows that for every value of F in the right-hand expression the time will be greater than for the corresponding value of F in the left-hand expression; and therefore the sums of the two sets of values, or the two integrals, cannot be equal. Hence F cannot be a function of time.

We have therefore only to consider variations in space. Now, if we confine our attention to one plane, we know that any variation of B 's place in that plane may be represented by a change in the values of x and θ ; where x is B 's distance from A , and θ the angle which the axis of x makes with some fixed line in the plane*. Then it is easy to show that F must not vary with θ . For if it does, let us suppose that when B has come to rest, and before it is allowed to return, it is made to rotate about A through an angle $d\theta$, and again brought to rest. Then the circumstances of A and B are unchanged; for the kinetic energy given to B during the rotation has been taken out again in stopping it. But if B is now allowed to return towards A , then, for every value of F in the right-hand expression, the value of θ will be greater or less by $d\theta$, the amount of the change, than for the corresponding value in the left-hand expression; and therefore, as before, the two integrals cannot be equal. Similarly, if we take coordinates x, θ, ϕ , in three dimensions, it will follow that F cannot be a function of ϕ .

Hence we are left with the conclusion that F can only be a function of r ; in other words, the force with which A acts upon B always tends towards A , and varies, if it varies at all, according to the distance from A only. But this is the definition of a central force.

[The proof just given, that F cannot vary with θ , appears

* We here make no assumption except that the force varies as B 's position in the plane varies; which is essential to every theory on the subject.

quite general. But it is easy to show that any particular law of force which can be imagined, other than that of a central force, is inconsistent with the conservation of energy. Thus, suppose the force to vary according to B's distance from some other point in the plane than A; then that distance can always be expressed in terms of the coordinates of its extremities, and therefore in an expression involving θ , which is inadmissible. Again, suppose the force to vary according to the perpendicular distance of B from some line in the plane. Then, if B move parallel to that line the force is constant, while if it be perpendicular it varies from zero; and it is easy to see that if B moves perpendicular to that line, and if, before it is allowed to return, it is rotated till the line AB is parallel to that line, then the two integrals will not be equal. Again, suppose the force to act upon a certain line only, so that when B is off that line no force acts upon it; then, if we suppose the return journey made parallel to that line, the energy on that journey is zero.]

We have throughout taken F as the force between A and B, resolved along the line joining them. We have still to consider the possibility of there being another component always at right angles to this line. This component, if it exists, will produce a rotation of B round A, which will increase B's kinetic energy; and as there will be nothing to balance it, this increase will go on for ever; so that the conservation of energy would not be true in this case.

I have thus proved, I believe, the proposition with which I started—namely, that the doctrine of central forces and that of the conservation of energy are mutually interdependent, so that one is not true without the other. In general, as remarked at the beginning, the existence of central forces is assumed, and the conservation of energy deduced from it. But the process may be reversed. The conservation of energy may be considered to rest, as a general law of nature, on the broad basis of observed facts, such as the conversion of heat, electricity, chemical actions, &c. into mechanical work, and the reconversion of mechanical work into these other forms of energy. There can be no doubt that the evidence of this character is of very great weight; and I am myself disposed to accept it as conclusive. But it must be pointed out that, unless the above investigation be false, it involves our accepting a mechanical definition of matter substantially to the following effect* :—“Matter consists of a collection of centres

* This definition has been already given in a pamphlet entitled ‘The Foundations of Mechanics’ (Charles Griffin and Co., 1882).

of force, acting upon each other according to laws which do not vary with time but do vary with distance."

This conception of matter is of course an old one, being that usually known by the name of Boscovitch. It has not, however, been generally accepted by writers on Mechanics; and in recent times certain special objections have been raised against it, which it seems well briefly to consider.

1. An objection, due to Professor Maxwell*, is that the conception does not comprise the idea of inertia, which is a fundamental fact with regard to matter. But when we say that a body has inertia, we simply mean that a finite force, acting upon it for a finite time, generates only a finite velocity. Hence it follows that any body we can see or feel, or know and investigate in any way, must have inertia; for suppose a body to possess no inertia, then the first time any force was applied to it, it would at once be removed to an indefinite distance, and would therefore be beyond the reach of investigation. To say that matter has inertia is therefore merely to assert the general principle that any thing our senses can deal with must be finite; and it is therefore a condition anterior to any theory of matter, not a part of such theory.

2. An objection given by Lamé† is that bodies, and especially homogeneous crystals, are not, within the limits of observation, denser at the centre than they are at the surface, which on the theory of central forces they apparently should be. But on this it may be observed as follows.

It may be admitted that collections of centres of force, *at rest under their mutual actions*, would be more dense towards the centre. We know no such bodies in nature. The nearest approach to it is the case of bodies so large that their molecular motions, and also their want of homogeneity, may be neglected in comparison of their mass. But the condition of large bodies does appear to agree with the theory; *e. g.* even the *mean* specific gravity of the earth (5·6) is greater than that of all bodies, except a few metals, at the surface. The want of homogeneity can have little influence at the temperature and pressure which prevail in the interior.

Again, it is known that, in all bodies, the actual centres of force must be bound up together in molecules so closely as to form coherent wholes, which no known force can change or break up. The relations in a crystal therefore are not those among separate centres of force, but among separate molecules.

Again, these molecules, being hot, are in rapid and continuous motion.

* 'Theory of Heat,' p. 85.

† *Elasticité des Corps solides*, p. 333.

Lastly, the laws of the forces of cohesion, whether in the interior of a molecule or between one molecule and another, are unknown.

In such circumstances, can it be held impossible that there should be laws of distribution of force such that in small bodies like crystals the difference in density at the centre and surface should be insensible? Lamé does not attempt to give any rigid proof that the uniform density of crystals (even if accurately true) is really incompatible with the theory of central forces. It is therefore merely a presumption, and a presumption which seems seriously weakened by the foregoing considerations; it cannot therefore be allowed to have any weight as against actual evidence.

3. An objection, due to Prof. Tait, is that we have no right to assume that force has any objective existence at all, or is any thing more than the rate of change of motion—and that in fact it cannot have an objective existence, because it can be affected with a positive or negative sign. But, with regard to the first part of this objection, a force is defined in Mechanics simply as a cause of motion; and therefore the remark is a mere denial of the general principle of causation. This is not the place to discuss the truth of that principle; but it may be observed that it is perhaps almost the only principle which may claim to have been accepted by all thinkers of all schools and in all ages. With regard to the second part of the objection, the circumstance that a force, or rather the symbol of a force, may be affected, for purposes of calculation, with a $+$ or $-$ sign is simply due to the fact that a force has a definite sense, or direction; and that direction is one of the properties of things to which the conception of positive and negative may properly be applied. For the same reason lines may be represented as $+$ or $-$, as in algebraical geometry; but they are not therefore regarded as non-existent. Nor is direction the only fact to which the conception applies; *e. g.* in treatises on algebra it is often pointed out that capital may be taken as positive and debt as negative. Will it therefore be argued that money has no real existence?

4. In some quarters an objection appears to be felt to the theory of central forces, on the ground that it involves the conception of action at a distance, which is supposed to be “unthinkable.” I am not aware that the term “unthinkable,” which is a new one, has ever been defined. Until it has been, it is impossible to say whether action at a distance is unthinkable, or whether the fact of a conception being unthinkable is sufficient reason, or any reason, for holding it to be untrue.

It seems desirable, before leaving the subject, to say a few words upon a theory which has been set up as a rival to that of central forces, and in some quarters has met with considerable favour. This theory supposes that bodies can act on each other only when in absolute contact; and that all the phenomena of the universe may be accounted for by the knockings together of a number of ultimate atoms, considered as very small impenetrable bodies, moving with high velocities in space.

It might be urged that before such a theory can be seriously discussed, it must be shown capable of explaining (as the theory of central forces certainly does explain) the facts and principles of Mechanics. I am not aware that this has been done. I may, however, point out that the theory is not inconsistent with the conservation of energy; that is to say, it can be reconciled with it by certain special assumptions. For the proof of that principle, as given above, does not necessarily imply that the forces acting are *continuous*. If the attraction of A be supposed to act on B by equal impulses at certain intervals of space, or distances from A, which distances remain always the same; then the proof will still hold; for B will be acted upon by exactly the same number of impulses, and at exactly the same places, on its return journey as on its outward journey, and the effects will therefore be the same. Now the "collision" theory above mentioned may be taken to represent the extremest possible case of this discontinuous action—there being then but one impulse, and that acting when A and B are in absolute contact.

Let us, however, consider the assumptions involved, if the conservation of energy is to hold in this extreme case. Imagine two "ultimate atoms," of equal mass, to meet each other with equal velocities in the same straight line. This is clearly a possible case under the theory; and the conservation of energy must therefore be consistent with it. Then the instant before the atoms meet they have no action upon each other, and the instant after, by symmetry, they must either be at rest or must have passed through one another. As the latter is contrary to the hypothesis, they must be at rest. Hence a finite mass moving with a finite velocity has been brought to rest in a space infinitely small; and therefore the impulse acting upon it must have been strictly infinite in amount. This collision therefore (and it is easily seen that the same will be true of all collisions) occasions the instantaneous development of a strictly infinite force. The atoms being brought to rest, there is no reason to be given why any thing further should happen. But we must assume it as an axiom that a further mutual im-

pulse is then given, sufficient (if the bodies are supposed perfectly elastic) to cause each to return on its path with a velocity exactly equal to that with which it arrived. This further impulse must also be instantaneous and infinite; for, force being the cause of motion, if the impulse were finite it would at once cause the bodies to separate through an indefinitely small space, and then, *ex hyp.*, no further action could take place, and the bodies would recede from each other with indefinitely small velocities. If, then, we make these three assumptions—(1) that there is an infinite impulse developed on the collision, which brings the atoms to rest, (2) that there is a further infinite impulse, which separates them, (3) that this further impulse, while infinite, is such as exactly to reverse the previous motion of each particle—then the conservation of energy may still be supposed to hold through the collision.

It remains to ask whether there are any advantages in the collision theory such as would warrant us in discarding the principle of continuity, and in making the somewhat violent assumptions described above. The advantages specially claimed by its advocates appear to be that it does away with the conception of action at a distance, and also with that of potential energy. The latter supposition, however, is not correct. At the instant when the two atoms are at rest their actual energy is zero, and the energy existing is entirely potential, being due to their capacity of generating a return velocity equal to that of arrival. Of the former supposition I have already spoken; and I may add that I have elsewhere* shown it to be impossible to explain certain elementary facts of physics without the hypothesis of action at a distance.

VI. *Relations between the Heats of Combination of the Elements and their Atomic Weights.*

To the Editors of the Philosophical Magazine.

GENTLEMEN,

THE periodic variation of the properties of the elements with their atomic weights is now accepted as proved by chemists; but no similar connexion seems yet to have been established between the atomic weights and heats of combination of the elements. Now, if the atomic weights of the elements are taken as abscissæ, and their atomic heats of combination with chlorine as ordinates of a curve, the heats of combination will be seen to be a periodic function of the atomic weights.

* "On Action at a Distance," Phys. Soc. 1881; Phil. Mag. Dec. 1880.

The curve thus produced may be divided into three principal series, with subordinate series between these. The curve is not complete, many of the elements theoretically required to form the fourth and fifth series not having yet been discovered.

The first four principal maxima correspond to every other element in Mendelejeff's first column—namely, lithium, potassium, rubidium, caesium. Of the other elements in Mendelejeff's first column, sodium occupies the first subordinate maximum, and copper and silver subordinate minima.

I enclose a copy of the curves for chlorine, bromine, and iodine. The numbers for the heats of combination are taken from the table in Naumann's *Thermochemie* (page 451), containing values for about thirty elements. Wherever possible, a compound containing two atoms of chlorine was chosen, and all other compounds were expressed in terms of two atoms of chlorine. Thus the number for KCl was multiplied by two, the number for Al_2Cl_6 was divided by three, &c. The numbers on the diagram are therefore the atomic heats of combination of the elements with equal weights of chlorine. The numbers for the heats of combination in presence of water have been preferred.

The heats of combination of rubidium, caesium, gallium, and indium with chlorine have not yet been determined; the curve is therefore represented by broken lines above strontium, barium, zinc, and cadmium.

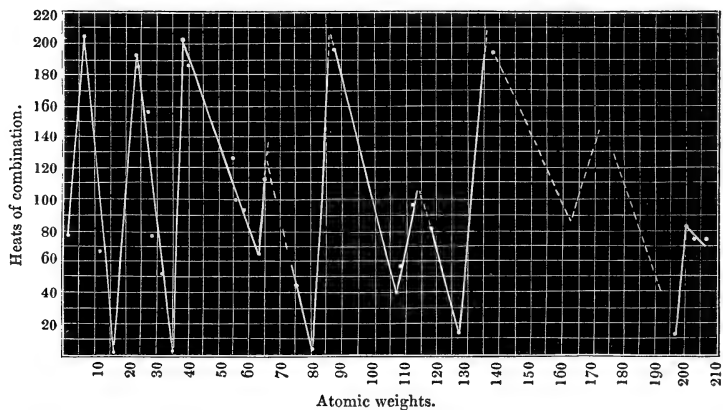
Small negative values are given in Naumann's table for some of the minima placed along the zero-line of the curve. The heat of combination of chlorine with chlorine is, of course, zero in terms of the other numbers.

The curve is drawn through among the observations, and not from point to point. This shows the curve better; and is quite justifiable when the inaccuracy of the heat-values (due to physical changes) is considered. The curves for bromine and iodine compounds agree very closely with the chlorine curve. An oxygen curve is very irregular, and does not seem to follow any law. Any one who will examine the tables for the heats of combination with oxygen will see how difficult it is to get numbers that have been taken under the same physical conditions.

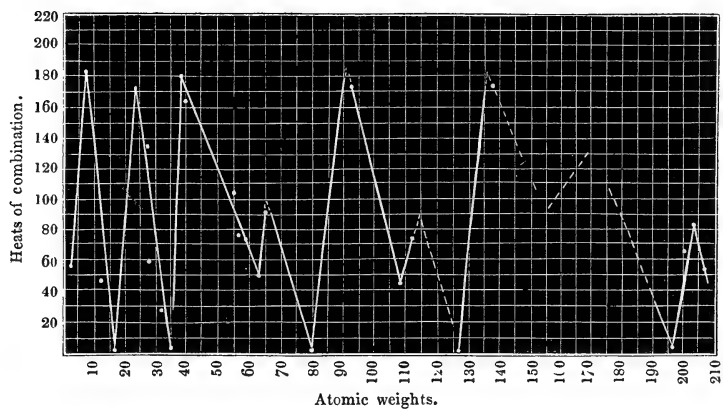
In the case of chlorine compounds, by taking the numbers expressing the heats of combination in presence of water we get most of the elements combining under similar conditions. The curve plotted from these values is more regular and symmetrical than the curve plotted from the values usually called the heats of combination with chlorine.

I can hardly believe the relations shown in these curves to

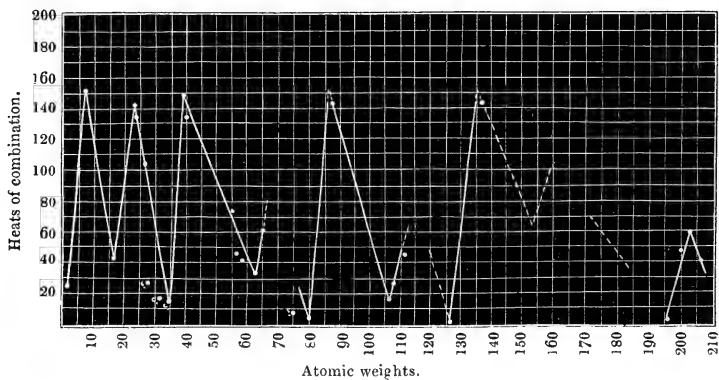
44 Mr. A. P. Laurie on the Relations between the Heats of
Chlorine.



Bromine.



Iodine.



be unknown; but I have not been able to find any account of such a relation, and therefore venture to send it for publication, as it seems to be of some interest.

I append a list of the elements plotted on the curves.

List of Elements in the Chlorine Curve.

	Name.	Atomic weight.
(i) (br)	Hydrogen	1
(i) (br)	Lithium	7
— (br)	Boron	11
(i) (br)	Oxygen	16
(i) (br)	Sodium	23
(i) (br)	Magnesium	24
(i) (br)	Aluminium	27
(i) (br)	Silicon	28
(i) (br)	Phosphorus	31
(i) (br)	Chlorine	35.5
(i) (br)	Potassium	39
(i) (br)	Calcium	40
(i) (br)	Manganese	55
(i) (br)	Iron	56
(i) (br)	Nickel	59
(i) (br)	Copper	63
(i) (br)	Zinc	65
(i) (br)	Arsenic	75
(br) (i)	Bromine	80
(br) (i)	Strontium	87
— (i)	Palladium	106
(br) (i)	Silver	108
(br) (i)	Cadmium	112
— —	Tin	118
(br) (i)	Iodine	127
(br) (i)	Barium	137
(br) (i)	Gold	196
(br) (i)	Mercury	200
(br) (i)	Thallium	204
(br) (i)	Lead	207

Those marked (i) appear in the Iodine curve, those marked (br) in the Bromine curve.

I am, Gentlemen,

Your obedient servant,

Cambridge (Caius College Laboratory),
November 1882.

A. P. LAURIE.

VII. *Amount of Carbon Dioxide in the Atmosphere.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the 'Philosophical Magazine' for November 1882, p. 388, Mr. Ernest H. Cook, in his paper on "Carbon Dioxide as a Constituent of the Atmosphere," has fallen into numerical errors in estimating the total amount of CO_2 existing in the atmosphere. Inasmuch as these erroneous results do not materially concern his very interesting discussion of the "Sources of Carbon Dioxide," they would not have called for any notice, but for the fact that his figures are put forward as *corrections* of the estimates made by his predecessors. I have not been able to discover the precise source of his errors in numbers; but they seem to have originated in the "cubic mile" estimates. It may be worth while to indicate the method of obtaining the correct numbers.

Let a = equatorial radius of the oblate spheroid;

b = polar " " " ;

h = height of homogeneous atmosphere enveloping it.

Then

volume of spheroid + volume of homogeneous atmosphere

$$= \frac{4}{3} \pi \times (a+h)^2 \times (b+h);$$

volume of spheroid or earth,

$$= \frac{4}{3} \pi \times a^2 \times b.$$

Taking Bessel's values of a and b for the terrestrial oblate spheroid, and assuming $h = 7990$ metres (Phil. Mag. 4th series, vol. xxvii. p. 16, Jan. 1864), we have

$$a = 6377398 \text{ metres,}$$

$$b = 6356080 \quad "$$

$$h = 7990 \quad "$$

Hence it follows:—

Cubic metres.

$$\text{vol. of earth + vol. of homog. atmos.} = 1,086,921,403,060 \times 10^9$$

$$,, \quad ,, \quad \text{itself} \quad . \quad . \quad . \quad . \quad = 1,082,841,791,538 \times 10^9$$

$$\therefore \text{vol. of homogeneous atmosphere} = \frac{1,086,921,403,060 - 1,082,841,791,538}{1} \times 10^9 = 4,079,611,522 \times 10^9$$

If we assume the CO_2 in the air = 4 vols. in 10,000 we have

$$\text{volume of } \text{CO}_2 = 1,631,845 \times 10^9 \text{ cubic metres or kilolitres.}$$

Now, as a cubic metre of CO_2 (density = 1.5291, air = 1) at Paris, under standard conditions, weighs 1.977467 kilogramme,

$$\therefore \text{weight of } \text{CO}_2 \text{ in atmos.} = 3,226,917 \times 10^9 \text{ kilogrs.}$$

The foregoing corrected numbers do not differ materially from the rough estimates of Dumas and Boussingault in relation to the volume of the homogeneous atmosphere, and of Roscoe and Schorlemmer in relation to the total weight of CO_2 in the air, as cited by Mr. Cook.

Similarly, if we assume the CO_2 in the air = 3 vols. in 10,000, \therefore volume of $\text{CO}_2 = 1,223,884 \times 10^9$ cubic metres or kilolitres, weight of $\text{CO}_2 = 2,420,188 \times 10^9$ kilogrammes.

We may estimate the weight of CO_2 in the atmosphere in a much simpler (and scarcely less exact) manner by computing its *pressure* on a unit surface. Thus, under standard conditions, the pressure of the atmosphere on a square metre at the surface of the earth = 10,332.9068 kilogrammes. Assuming the amount of CO_2 in the air = 4 vols. in 10,000, its *weight* or pressure would be 6.1164 parts in 10,000. Hence the *pressure* of CO_2 on one square metre at the earth's surface is equal to 6.320019 kilogrammes.

Now the surface of the terrestrial oblate spheroid, of the dimensions above given, = $509,950,861 \times 10^6$ square metres. Hence the pressure of CO_2 on the entire surface of the spheroid = $3,222,900 \times 10^9$ kilogrammes.

And, similarly, assuming the amount of $\text{CO}_2 = 3$ vols. in 10,000, its weight or pressure would be 4.5873 parts in 10,000. Hence the pressure of CO_2 on the entire surface of the spheroid would be = $2,417,175 \times 10^9$ kilogrammes.

I am, yours sincerely,

JOHN LECONTE.

Berkeley, California,
November 30, 1882.

VIII. *Apparent Attractions and Repulsions of Small Floating Bodies.* By JOHN LECONTE*.

[Plate I.]

ALTHOUGH the apparent attractions and repulsions of small floating bodies is one of the most familiar phenomena, and one of the earliest to which the physical theory of capillarity was applied, yet it remains a perplexing puzzle to a large number of intelligent students. This arises from the fact that the popular explanations given in many standard works on elementary physics do not bear a critical examination, and are consequently any thing but satisfactory to the student who endeavours to secure a clear physical conception of the cause of these motions.

* From Silliman's American Journal for December 1882.

This class of phenomena seems to have been first explained by the celebrated Mariotte about 1655; but more particularly by the great geometer Monge*, who distinctly and correctly referred them to the action of the surface-film of the liquid, as modified by the presence of the partially immersed solid bodies. In more modern times the improved theory of capillary action of Young† and Laplace‡, as modified by the refined physico-mathematical investigations of Gauss§ and of Poisson||, refers all capillary phenomena not only to the reciprocal attractions between the liquid and the solid, but also to the existence of a tense superficial film at the free surface of every liquid, which gives origin to a tensile elastic reaction resulting in the development of a force tending to elevate or depress the liquid according as its terminal surface adjacent to the solid is concave or convex. The actual existence of such an elastic contractile film at the bounding surfaces of liquids is abundantly verified by numerous conclusive experiments with films of soapy water, as well as by the whole class of striking phenomena rendered prominent by the admirable researches of Plateau¶, and the equally satisfactory investigations of Dupré** and of Quincke††.

Even according to the more exact mathematical theories of capillarity of Laplace and of Poisson, the explanation of this class of phenomena is not altogether free from ambiguity. Thus, in Laplace's investigation, as the pressure of the atmosphere appears as a prominent element in producing the motions of such floating bodies, the student is naturally perplexed when he is confronted by the somewhat awkward fact that capillary phenomena are entirely independent of the pressure to which the apparatus is exposed, as was long ago proved by the experiments *in vacuo*, executed by the members of the Accademia del Cimento of Florence. It is proper to add, however, that a critical examination of the explanation given by Laplace, as well as by Poisson ‡‡, very clearly indicates that

* *Mémoires de l'Acad. des Sciences* for 1787, p. 506 *et seq.*

† Phil. Trans. for 1805, p. 65 *et seq.*, on the "Cohesion of Liquids."

‡ *Mécanique Céleste*, tome iv., "Supplément au Livre x.," "Sur l'Action capillaire" (1806). Also "Supplément à la Théorie de l'Action capillaire" (1807).

§ *Principia Generalia Theoriæ Figuræ Fluidorum in Statu Æquilibrîi*: Göttingen, 1830.

|| *Nouvelle Théorie de l'Action capillaire*: Paris, 1831.

¶ *Statique Expérimentale et Théorique des Liquides*.

** *Théorie Mécanique de la Chaleur*, chapitre ix., "Capillarité," p. 206 *et seq.*: Paris, 1869.

†† Phil. Mag. [4] xxxviii. p. 81, and xli. pp. 245, 370, 454; [5] v. pp. 321, 415, and vii. p. 301.

‡‡ Laplace, *Mécanique Céleste*, tome iv. "Supplément au Livre x." art. 11, p. 41 *et seq.* Poisson, *op. cit. supra*, arts. 81-85, pp. 162-173.

when the effective forces are considered, the pressure of the atmosphere is practically eliminated from their equations ; so that finally the forces actually in operation which produce the tendency of such bodies to approach or to recede from one another are due exclusively to molecular actions.

Nevertheless, inasmuch as Laplace's explanation contains the pressure of the atmosphere as a term, while at the same time it makes the effective force equivalent to a modification of hydrostatic pressure, which is negative or positive according as the surface of the liquid adjacent to the solid bodies is concave or convex; it is by no means surprising that the idea of atmospheric pressure should have been associated with these phenomena. Hence we find that many first-class physicists, such as Lamé, Desains, Jamin, Everett*, and others introduce the pressure of the atmosphere as a fundamental element into their explanations of these motions. Indeed, Laplace himself seems to have been impressed with the apparent conflict between theory and experiment; for after giving the result in relation to the case of two solids moistened by the liquid (in which the hydrostatic pressure between them has a negative value), he significantly adds:—" Dans le vide les deux plans tendraient encore à se rapprocher ; l'adhérence du plan au fluide, produisant alors le même effet que la pression de l'atmosphère "†.

Doubtless the view which ascribes these apparent attractions and repulsions to the modifications of hydrostatic pressure due to the action of capillary forces is a philosophical one, and capable of being put into a mathematical form ; yet when the physical cause of the disturbance of hydrostatic equilibrium is kept in the background, the student is greatly embarrassed and perplexed in obtaining clear conceptions of negative and positive pressures, complicated as they are with considerations relating to the pressure of the atmosphere, especially when he is assured that the last-indicated pressure must be inoperative, from the experimental fact that the phenomena take place *in vacuo*. Moreover, from this point of view, he is liable to lose sight of the real physical cause of all capillary phenomena, viz. the reaction of the tense superficial film of the liquid—the true and efficient cause of the disturbance of hydrostatic

* Lamé, *Cours de Physique*, 2nd ed. (Paris, 1840), tome i. p. 188 *et seq.* art. 143. Desains, *Traité de Physique* (Paris, 1857), tome i. pp. 603–606. Jamin, *Cours de Physique*, 3rd ed. (Paris, 1871), tome i. pp. 229, 230. Everett, translation of Deschanel's *Traité Élémentaire de Physique* (N.Y., 1880), part i. p. 136, art. 97 D. Péclet, in his *Traité de Physique* (4th ed. Paris, 1847, tome i. p. 158, art. 224), leaves out the pressure of the atmosphere, and ascribes the motions exclusively to negative and positive hydrostatic pressures.

† *Mécanique Céleste*, Supplément au Livre x. art. 11, p. 44.

pressure. It seems to me that, by referring the motions of such bodies directly to the action of this tensile superficial film, a fundamental principle in the physical theory of capillarity is secured in the mind of the student; while the resulting disturbances of hydrostatic equilibrium are not primary facts, but secondary consequences of the more fundamental cause.

The general explanation which I am about to offer of the "apparent attractions and repulsions of small floating bodies" is so simple and obvious a deduction from the fundamental laws of capillary action, as expounded by Laplace and Poisson, that it is difficult to bring myself to believe that it has hitherto escaped the attention of physicists. Nevertheless I have not, thus far, been able to find it in any of the treatises on physics.

In order to render my explanation more clear, it will be necessary to present the commonly received popular explanations of these phenomena, and to indicate their defects.

Two methods of experimental illustration may be adopted, viz.:—1st, by floating in water two small bodies whose surfaces have been so prepared as to be moistened or non-moistened by liquid; and 2nd, by plunging vertically into water or into mercury two parallel plates of clean glass suspended by threads.

Ordinary Popular Explanations.

Case 1. When both bodies are moistened.—In this case (Plate I. fig. 1, A, B, and A' B'), when the two bodies are brought so near that their intervening concave menisci join each other, the bodies are drawn together by the weight of the column of the liquid m , elevated above the level ab , acting like a loaded cork secured to each of the bodies.

Case 2. When both bodies are not moistened. In this case (fig. 2, A, B, and A' B'), when the two bodies are brought so near that the intervening convex menisci unite, e and g are more depressed than d and f ; consequently the two bodies are pushed toward each other by the greater exterior hydrostatic pressure exercised by the portions dh and fx .

Case 3. When one body is moistened and the other is not moistened.—In this case (fig. 3, A, B, and A' B'), if the moistened body B or B' were alone, the concave meniscus would be elevated to o ; in like manner, if the non-moistened body A or A' were alone, the convex meniscus would be depressed to r . Now if the two bodies are brought so near that their menisci join each other, the intervening liquid surface will take an intermediate position nk . Hence the point n will be below the point o ; and the point k will be above the point r . It is

therefore assumed that the moistened body B or B' will be drawn away from A or A' by the excess of the weight of the exterior above the interior meniscus, due to the difference of height on or $o'n'$. In like manner, the non-moistened body A or A' will be pushed away from B or B' by the excess of interior hydrostatic pressure due to the difference of level kr . Hence there is apparent mutual repulsion.

Defects of the foregoing explanations.—Leaving out of consideration those physicists who have adopted, more or less completely, the mathematical methods of Laplace and of Poisson, the foregoing seem to be the generally received popular explanations of this class of capillary phenomena. They are those given by Brewster, Daguin, Silliman, Snell, and other writers on elementary physics, and are essentially identical with the explanations originally proposed by Monge*. The most cursory examination will serve to show their unsatisfactoriness.

In the first place, each case requires a special explanation: there is no common physical principle coordinating the three cases under consideration. Thus, in case 1 the weight of the intervening elevated column of liquid draws the bodies together, without reference to the modification of hydrostatic pressure due to the elevation. On the other hand, in case 2 the bodies are pushed together by the excess of the exterior hydrostatic pressures. Finally, in case 3 it will be noticed that the excess of hydrostatic pressure due to the difference of height equivalent to on or $o'n'$ is made a pulling force, urging B or B' to the right; while the excess of hydrostatic pressure due to the difference-height equivalent to kr is made a pushing force urging A or A' to the left. Now, why this difference in the direction of action of the excess of hydrostatic pressures? Why not regard the excess of pressure on the right of B or B' (equivalent on or $o'n'$) as a pushing force urging B or B' towards A or A'?—a result which is evidently at variance with experiments.

In the second place, it is very clear that the laws of hydrostatics are so seriously modified by the action of capillary forces (the disturbances of level being in fact due to them), that it is very questionable whether hydrostatic pressure can

* Brewster, *Encyc. Britannica*, 8th ed., art. "Hydrodynamics," chap. iii. "On Capillary Attraction and the Cohesion of Liquids." Daguin, *Traité Élémentaire de Physique*, 3rd ed. (Paris, 1867), tome i. art. 226, pp. 209, 210. Silliman, 'Principles of Physics,' 2nd ed., revised and rewritten, (Philad. 1861), art. 242, pp. 195–196. Snell's Olmsted's 'Nat. Phil.' 2nd revised ed. (N. Y. 1870), art. 229, pp. 150, 151; also Kimball's 3rd revised ed. (N. Y. 1882), art. 202, p. 135. Monge, *Mém. de l'Acad. cit. anté.* Of the above, Daguin gives the most explicit and clear statement of these explanations.

be properly or safely invoked to explain these phenomena without the restrictions imposed by the introduction of the negative and positive molecular pressures, which constitute such important factors in the physico-mathematical analyses of these questions.

Proposed General Explanation.

In the following explanation of the "apparent attractions and repulsions of small floating bodies," I have referred this class of phenomena to two fundamental principles of capillarity which are abundantly verified by observation and experiment, viz.: 1st, that in every case, whether of moistened or non-moistened bodies, there exists an adhesion between the solid and the liquid; and 2nd, that the capillary forces are, in any given case, inversely proportional to the radii of curvature of the menisci, and their resultants are directed toward the centres of concavity. It seems to me that these two fundamental and well-established principles of capillary action will explain the whole class of phenomena in a much more consistent and satisfactory manner.

Case 1. Fig. 1. Before the two bodies are brought near each other, the concave meniscus around each of them having the same radius of curvature on all sides, each of the floating bodies is in equilibrium under its action. But when brought so near that their menisci join each other, the radius of curvature of the united intervening concave meniscus at *m* (fig. 1) is less than that of the exterior concave menisci at *a* and *b*, and its superior tension acts upon both bodies toward a common centre of concavity at *5*. Hence, by virtue of the smaller radius of curvature of the intervening tense film, the interior forces prevail, and the two bodies are drawn together.

Case 2. Fig. 2. The same explanation applies to this case. The common or united intervening convex meniscus being attached to the bodies at *e* and *g* (fig. 2), has a smaller radius of curvature than the exterior convex menisci at *d* and *f*. Hence in this case likewise, by virtue of the smaller radius of curvature of the intervening contractile elastic film, the interior forces necessarily prevail, and the two bodies are drawn together.

Case 3. In this case (fig. 3) it is evident that as the centres of concavity of the interfering menisci are in opposite directions, the common or united meniscus formed by their union, as *kn*, must have a radius of curvature greater than that of either of the exterior menisci at *c* and *o'*. Hence, by virtue of the smaller radius of curvature of the exterior tense elastic films at *c* and *o'*, the exterior capillary forces must

prevail, and the two bodies are drawn apart by the superior tensile reactions directed toward the centres of concavity at 3 and at 4 (fig. 3).

It will be noticed that in the preceding explanations of this class of capillary phenomena I have referred the apparent attractions and repulsions exclusively to the elastic reactions of the tense surface-film, whose form is modified by the proximity of the partly immersed solid bodies. For the reasons previously assigned, I have left out of consideration the modifications of hydrostatic pressure, which are, after all, really due to these elastic reactions. To those physicists who prefer the mathematical methods of Laplace and of Poisson, my explanations may seem to be less complete and exhaustive; but in such general explanations it is of primary importance to keep steadily before the student the fundamental physical principle which constitutes the *fons et origo* of these phenomena.

There is, however, an objection to the explanation which refers this class of capillary phenomena to the contraction of the tensile film of liquid adjacent to the sides of the partly immersed solids, which it is proper to notice. Since the capillary force thus developed is inversely proportional to the radius of curvature and directed toward the centre of concavity, it has been urged that when an isolated vertical plate that has its two parallel faces of different substances is partly immersed in the liquid under such conditions that the radii of curvature of the menisci on the opposite faces are unequal, there should result a difference of pressure; so that such an isolated body floating on an indefinite surface of a liquid would, under the mutual action of the fluid and solid, take on a horizontal and perpetual motion of translation. There are obvious mechanical difficulties in the way of the admission of such a result; for, as suggested by Poisson, in such a movement the centre of gravity of the entire system is not displaced. Laplace seemed to think that there would be some difference of pressure in such cases, but that it would be so small that it might be neglected*. It is evident that, however small it might be, a motion of translation would be the consequence; and this it seems difficult to admit. Dr. Thomas Young, in a letter to Poisson, insists upon this as a most serious objection to Laplace's theory of Capillarity†. On the contrary, Poisson shows that his own modified theory does not lead to this mechanical difficulty; for it indicates that, under the foregoing conditions, the horizontal pressures on the opposite faces exactly counter-

* *Supplément à la Théorie de l'Action Capillaire*, p. 43.

† Poisson, *op. cit. antè*, art. 128, p. 265.

balance each other, so that the floating solid can have no motion of translation*. In like manner, Dupré, in his admirable researches on "Molecular Actions," investigates this anomalous deduction from Laplace's theory, indicating the completeness of the latter, and showing that Poisson is correct in the conclusion that the horizontal molecular forces exactly balance one another†.

Notwithstanding the *à priori* mechanical impossibility of an isolated floating body taking on a motion of translation under the conditions above designated, I made the following arrangements with the view of experimentally testing the question. A plate of well-cleaned glass (fig. 4), gg' , and a plate of polished steel, ss' , were cemented together so as to constitute a single compound plate of these substances. This was floated in a vertical position by securing masses of cork c and c' to the two faces, and attaching a leaden sinker (w) of the proper weight to the lower extremity of the plates. If such a composite plate is plunged vertically into alcohol contained in a large vessel, a concave meniscus will be formed by the ascending liquid on the glass face, while on the steel face no meniscus will be formed, and the adjacent surface of the alcohol will be horizontal. Now Dr. Young insisted that, according to Laplace's theory, the horizontal molecular pressures on the opposite faces being unequal, the composite plate should be drawn in the direction of the centre of concavity of the meniscus on the glass face, and thus cause the entire system to take on a motion of translation toward the right. The arrangement represented in fig. 4 was so sensitive that it was difficult to avoid the influence of slight currents of air; nevertheless there were no indications of the action of unbalanced forces on the floating apparatus. Hence the idea that such a system would take on a horizontal and perpetual motion under the action of molecular forces is not only inconsistent with fundamental mechanical principles, but is contradicted by direct experiment; for no motion takes place in such a composite vertical plate when partly immersed and floating in the liquid‡.

Moreover it seems to me that, according to any theory of capillarity which is based upon the action of a tense elastic superficial film, it is clear that the tensile reaction of the

* Poisson, *op. cit. antè*, arts. 85 and 96, pp. 172 and 194.

† Dupré, *op. cit. antè*, chap. ix. pp. 396-400.

‡ It is scarcely necessary to add that the irregular and perplexing capillary motions due to the difference in the surface-tensions of different liquids or of the same liquid in different states are here excluded from consideration. The curious motions of small isolated masses of camphor, when floating on the surface of warm water, come under this category. These phenomena have been carefully studied by Tomlinson and others.

bounding film which envelops such a floating composite plate must necessarily be exactly the same in all parts of its perimeter; so that it is impossible for the forces due to capillary actions to disturb the equilibrium of such a body while floating in the liquid so long as its component plates are maintained at a fixed distance apart. It is obvious that the tensile reaction can only tend to press the plates together; it cannot produce a motion of translation. It is almost needless to add that in the cases previously considered, where the floating solids were separated and movable, the conditions of equilibrium were disturbed by the modifications of the interfering menisci due to their proximity; and these conditions of equilibrium could only be realized by mutual contact, or by recession beyond the reach of disturbing influences.

It is likewise evident that in cases 1 and 2 the interfering and modified united bounding films tend in each case to assume a minimum perimeter, which is only secured when the two bodies are brought into contact. In the composite plate above considered this condition is instantly realized upon partial immersion in the liquid. This principle explains why it is practically so much more difficult to experimentally verify case 3 than cases 1 and 2. For in case 3, when the floating bodies are brought so near to one another that the interfering menisci form a common enveloping film, the principle of minimum bounding perimeter prevails, and the verification of apparent repulsion fails; for the two bodies are drawn together as in cases 1 and 2. The fact that in such cases floating bodies apparently repel one another at a certain distance; but on nearer approach apparently attract, is noticed both by Laplace and by Poisson as a deduction from their respective theories of capillarity, and was experimentally verified by the Abbé Haüy and others.

Finally, it may be proper to add that the reaction of the surface-tension of liquids always tends to reduce the surface to the smallest area which can be enclosed by its actual boundary. This fundamental principle of the tensile reaction tending to reduce the bounding area or bounding perimeter to a minimum affords a very simple and elegant explanation of the whole class of phenomena under consideration*. As already intimated, cases 1 and 2 evidently come under this principle; for the common bounding perimeter produced by the union of the menisci, in tending by virtue of its elastic reaction to become

* This principle is very elegantly deduced by the great geometer Gauss by the application of the principle of mutual velocities. He shows that the condition of capillary equilibrium is that the expression for the force-function shall be a minimum (*vide op. cit. antè*).

a minimum, draws the two floating bodies together. The same is true of case 3, provided the floating bodies are brought into such proximity that a common bounding perimeter is produced by the interfering menisci. But when the proximity is not sufficient to secure this condition, the disturbance of equilibrium due to this interference results, as we have seen, in the recession of the floating bodies. For in the latter case the first effect of the interfering menisci (as previously shown) is to augment the radius of curvature at the intersecting portions; this is evidently equivalent to a tendency to increase the bounding perimeter of the menisci enveloping each of the bodies; so that the minimum principle operates to separate them.

Hence the two fundamental principles of capillarity—1st, that the elastic reaction is inversely proportional to the radius of curvature of the meniscus, and 2nd, that the same contractile reaction tends to reduce the perimeter to the smallest which can be enclosed by its actual boundary, are coordinated, and alike concur in furnishing complete explanations of this class of phenomena, without the necessity of invoking the agency of atmospheric pressure or the modifications of hydrostatic pressure due to the operation of these molecular forces.

Berkeley, California, October 10, 1882.

IX. *Notices respecting New Books.*

Physics of the Earth's Crust. By REV. OSMOND FISHER.
London: Macmillan. (Pp. xiv + 299.)

THIS work is not a mere reprint of papers well known to many of our readers, some of them having been printed in our columns, though free use has been made of them. The author has been "for many years past convinced that various questions of Physical Geology might be answered negatively, if not positively, by applying to them simple mathematical reasoning and quantitative treatment." He candidly confesses that his views, "in some respects, have been greatly altered by the application of this method." In evidence of this he expressly states that "the views now put forward differ in some respects from those which I have previously published in contributions to scientific periodicals. But the effect of the progressive application of the quantitative method may be traced in the book itself; and the development of ideas, proposed in the earlier chapters, will sometimes be found to have taken an unexpected turn later on. This remark applies especially to certain hypotheses, which at first presented themselves in a favourable light, to account for compression and for the formation of ocean-basins." The same spirit is seen in what follows:—"On a review of what I have written, I feel how many difficulties have

been left unsolved, and some which have occurred to me not even mentioned."

The first question discussed (Chap. I.) is the underground temperature*; and a result given as approximately true *near the surface* is, in agreement with the view, we believe, now generally adopted, that the law of increase of temperature is "on the whole an equable one, amounting on an average to about one degree Fahrenheit for every 51 degrees of descent."

Mr. Fisher's theme is the Physics of the Earth's *Crust*; for if we know little comparatively of this, much less do we know of the condition of the interior of the earth. Chapter II. deals specially with this subject; and in it we have an interesting discussion of the views of Sir W. Thomson, Messrs. Delaunay, Hopkins, and others. Is any portion at the present date liquid? and if so, how much of it? As a matter of comparative certainty, Mr. Fisher, in agreement with other physicists, infers from the present form of our globe, and from the law of variation of gravity on its surface, that it was once wholly melted, and now consists of concentric spheroidal shells, each of equal density throughout and of definite form (*cf.* J. D. Forbes, § 256 &c., 'Progress of Mathematical and Physical Science,' and Col. Clarke's 'Geodesy,' cap. iv. & xiii.). We give here the author's summing-up in partial opposition to Sir W. Thomson's hypothesis of a rigid earth:—"It does not appear necessary that the earth should be absolutely solid from the centre to the surface to satisfy the requirements of great rigidity as a whole. May this not be satisfied by the hypothesis of a rigid nucleus nearly approaching the size of the whole globe, covered by a fluid substratum of no great thickness compared to the radius, upon which a crust of lesser density floats in a state of equilibrium?" On the other side we may cite Prof. Green ('Nature,' March 23, 1882), who considers that Mr. Fisher's work "seems rather to show that the earth did not consolidate in the way supposed by Sir W. Thomson," and that there are other equally probable hypotheses as to the way in which the earth may have passed from a fluid to a solid state. In Chapter III. we have a discussion of the internal densities and pressures—corrections of Waltershausen's theory, and fluid pressures within the earth calculated from Laplace's law of density. Chapter IV. treats of the lateral pressure, and examines the generally received views of the effect of lateral pressure on the contortion of rocks. Chapter V. examines the truth of the assumption that, because the pressure thus produced would be sufficient for the purpose, the work has been accomplished in this way. The next Chapter is principally occupied with a criticism of Sir W. Thomson's paper "On the Secular Cooling of the Earth," and an account of Mr. Mallet's experiments. The following chapter (VII.) summarizes the results of this, and con-

* Our author commences with this discussion, because "the distribution of heat in the interior of the earth is one of the cardinal conditions upon which all physical questions connected with it depend."

cludes that "the large discrepancy (pointed out in chap. vi.) shows that the hypothesis, that the inequalities of the surface have been caused by the cooling and consequent contraction of the earth regarded as a solid globe, is untenable." Hence the author thinks we are shut up to one or both of the following alternatives: "either the inequalities of the surface are not altogether, or even chiefly, due to lateral compression; or there has been some other cause involved in producing the inequalities of the crust other than the contraction of a solid globe through mere cooling. . . . The shifting of the crust towards a mountain-range, which is testified by the corrugation of the rocks of which it is formed, requires a more or less fluid substratum to admit of it. The sinking of areas such as deltas, and other regions of deposition, demand a like arrangement; and, in short, it appears that the crust, in the form in which it exists, must be in a condition of approximate hydrostatical equilibrium, such that any considerable addition of land will cause any region to sink, or any considerable amount denuded off an area will cause it to rise." In Chapter VIII. the hypothesis of a fluid substratum is discussed in connexion with Mr. Hannay's recent experiments; in Chapter IX. the supposition of an inflexible crust is dismissed; and in Chapter X. the analogy between the condition of the crust and the case of a broken-up area of ice, refrozen and floating upon water, is worked out to the conclusion that "the crust is certainly less than 30 miles thick." In Chapter XII. we get the result, "The crust of the earth at a place near the sea-level is about 25 miles thick; beneath the central parts of the ocean it is about 20 miles thick, or perhaps less, and that it is more dense in those regions"*. The contraction is next calculated on the two suppositions that oceans are due (1) to compression, (2) to a denser crust; and the outcome of the discussion is that the ocean-basins are thought to be due to a greater density and general depression of the suboceanic crust. Then it is shown that the oblateness may have been altered by changes in the rotational velocity; but Mr. Fisher thinks there is no indication in the arrangement of mountain-chains of their having been connected with such a mode of action. In Chapters XV. to XXI. the alternative hypothesis is examined, viz. that the causes of compression are situated within the crust itself. We content ourselves with giving the titles of these Chapters:—Compression and Volcanic Action (Prof. Nordenskiöld's theory and the views of American geologists are amongst the matters treated of in this chapter); on Faulting; Geological Movements explained; Mr. Mallet's Theory of Volcanic Energy (opposed to Mr. Fisher's results); the Volcano in eruption (discussion of theories of Scrope, Dalton, Mallet, and Richthofen); Sequence of Volcanic Rocks; and, lastly, Geographical Distribution of Volcanos.

Mr. Fisher closes with reminding his readers that his work "deals with a great number of questions, each in a very superficial

* The densities given are—suboceanic 2·955, continental 2·68.

manner, and that indeed entire treatises might be written upon the subject of some chapters. . . . There are phenomena which appear to require for their production causes hitherto unexplained, which must be looked for possibly outside the globe itself. Such are climatal changes. Such is also the remarkable fact that the grand efforts of elevatory action appear to have been paroxysmal, and after slumbering for long ages to have been more intense at certain periods, the last of which was subsequent to the Eocene. These questions are left for a rising generation of physicists and astronomers, who, it is hoped, may think the ascertained facts of Geology a worthy field for the application of exact scientific methods."

There is a good summary of the contents of the several chapters, and an Appendix, which takes notice of two memoirs (unpublished at the date of publication of the 'Physics')—one by Mr. G. H. Darwin, "On the Stresses caused in the Interior of the Earth by the Weight of Continents and Mountains," and the other by M. Ed. Roche. An Index closes the volume.

The Solution of the Pyramid Problem. By ROBERT BALLARD. New York: John Wiley and Sons. 1882. (109 pp., 75 woodcuts.)

THIS is another addition to the "curious" literature about the great pyramids. Taking the principal pyramids at Chizeh, the author states that the lines joining their centres form, together with the meridional and east-to-west lines through them, a system of right-angled triangles whose sides are in simple ratios, *e. g.* as 3 : 4 : 5, or as 20 : 21 : 29, &c. Also the sides of these triangles are said to contain each an exact number of a new cubit, styled by the author "R.B. cubit" [query, "Robert Ballard cubit"?], so called "because it closely resembles the Royal Babylonian cubit:" it seems also that this "R.B. cubit" is exactly one geodetic "third," *i. e.* $\frac{1}{60}$ th part of one second of the earth's polar circumference. A new theory is advanced, that the pyramids were intended as a sort of fixed surveying-instrument, *viz.* as great marks for ranging out lines on definite bearings, and for recovering landmarks lost in the annual inundations of the Nile: this may be so, but they would be a very expensive form of landmarks. A whole chapter is devoted to the advantages to surveyors and engineers of the use of right-angled triangles, especially of the "beautiful triangle" whose sides are as 20 : 21 : 29. Another chapter is devoted to the "beautiful symbol" of the pentangle (or 5-pointed star), which it seems is the "geometric emblem of extreme and mean ratio," and also the "symbol of the Egyptian pyramid Cheops" (the pyramid, however, is four-sided). Moreover, if "we close the geometric flower Pentalpha," and thereby "convert it into a pyramid," it appears that "such transitions point to the indissoluble connexion between plane and solid geometry." This work is handsomely got up.

ALLAN CUNNINGHAM, *Major R.E.*

Saturated Steam the Motive Power in Volcanoes and Earthquakes ; great Importance of Electricity. By R. A. PEACOCK, C.E. &c.
London : E. and F. N. Spon. 1882. (136 pp.)

THE object of this little work is (as expressed in the title) to show that saturated steam is the motive power in volcanic eruptions and earthquakes. Evidence is quoted from various authorities to the effect that (1) free hydrogen has been detected in the flames issuing from certain volcanoes; (2) the temperature of combustion of hydrogen with oxygen is $14,541^{\circ}$ F.; (3) clouds of steam are frequently ejected from volcanoes; (4) the pressure of saturated steam at $14,541^{\circ}$ F. is about one million tons per square inch. Hence the conclusion that "there is no other terrestrial force at all approaching to saturated steam in power and magnitude," and the question (about volcanic eruptions and earthquakes) "Will any man venture to deny that saturated steam was the active agent on these occasions?" answered further on thus:—"This steam *necessarily* causes earthquakes and volcanoes; for how is it possible so very vast a power can remain idle?"

In part evidence of the great resistance of the air to a body in rapid motion, a graphic account is given of the author's personal experience thereof on a runaway horse forty years ago. In other cases statements of fact are generally freely supported by quotations from authority; the author even thinks it as well to "prove by a quotation from 'Chemistry' that, since air and water were present in the volcano of Java, oxygen and hydrogen were necessarily present also." The figure $14,541^{\circ}$ F. quoted above seems very precise in the units, but it is quoted (as 8061° C.) from Prof. Tyndall with the remark that "Professor Tyndall is a safe man to follow."

The style is not always clear; *e. g.* the phrase "being about equally and very great" occurs thrice; and the terminology is sometimes loose; *e. g.* steam is described as a "force" (*v. supra*).

ALLAN CUNNINGHAM, *Major R.E.*

X. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xiv. p. 478.]

November 15, 1882.—Dr. J. Gwyn Jeffreys, F.R.S.,
Vice-President, in the Chair.

THE following communications were read:—

1. "The Drift-beds of the North-west of England and North Wales.—Part 2. Their Nature, Stratigraphy, and Distribution." By T. Mellard Reade, Esq., C.E., F.G.S.

The author stated that the first part of this paper, read in 1873, treated of the low-level Boulder-clay and sands, specially in relation

to the contained shells. Since that time he has been diligently collecting information to enable him to treat of the nature, origin, and stratigraphy of the Drift lying between Liverpool and St. Bees, and Liverpool and Caernarvonshire. He finds that, in the basin of the river Mersey, the Triassic rocks underlying the low-level Boulder-clay and sands are cut up by a system of preglacial valleys, in some cases presenting very precipitous sides and not in all cases following the present course of the rivers. If the mantle of clay and sands could be stripped off, we should have scenery differing considerably from the present surface-features. These preglacial valleys are, in parts of their courses, considerably below the present low-water level.

Where the rock has been bared and is of a nature capable of retaining striations, we almost invariably find it planed and grooved in a direction approximately from N.W.; and when the rock is soft, it is broken up into rubble and red sand.

Upon this débris of the Trias lie the low-level Boulder-clay and sands of the plains, the clay lying immediately on the rock being frequently, but not invariably, of a sandier and harder nature than the upper beds. Lines of erosion of a local nature, but often of considerable extent, often occur at the top of this clay and then die out; or there are thin or thick beds of sand and gravel intercalated at the junction and also dying out. Sometimes sand and gravels underlie this harder clay; but the larger mass of the low-level clay is of a more plastic nature, and is used in brickmaking. Intercalated sand-beds also occur in this; and sometimes the clay gets stonier again near the top.

If we trace the drift from the sea up each river-valley to the high lands, we see at once that the nature of the clay gets more intimately connected with the rocks in the basin above. This is specially noticeable in the Ribble valley, where the brown marine Boulder-clay gradually, above Milton Bridge, gets replaced by a drift composed almost wholly of the débris and grindings of the Carboniferous limestone and grits above. In the mountain-districts, also, the drift becomes more localized both in Cumberland and Wales.

The author's conclusions are that an ice-sheet, radiating from the mountain-district of the English lakes and the south of Scotland, produced the planing and grooving of the rock and the red sand and rubble débris; then the ice melted back into local glaciers, and the submergence began. The low-level Boulder-clay and sands were, during a slow submergence, laid down probably at depths of from 200 to 300 feet; and the author considers that all the phenomena can be satisfactorily accounted for by ordinary river-action and fraying of the coasts by the sea, combined with frost and ice due to a severer climate bringing down the materials of such river-basins to the sea, while icebergs and coast-ice sailed over, dropping on the sea-bottom their burdens of erratic stones and other materials from the mountain-districts of the north. He pointed out, also, that the great majority of the well-glaciated rocks were specially

those that could be traced to the high lands. This fact was forced upon his notice after making a large collection of glaciated boulders and pebbles. Among the rocks he had been able to identify, with the help of Professor Bonney and Mr. P. Dudgeon, of Dumfries, Scawfell granite (Eskdale of Mackintosh) was the most abundant granite; then came grey granites from Dumfries; syenite from Buttermere, which occurred all over the area described, and up to 1200 feet on the Macclesfield Hills; and syenite from Cannock fell. Other probable identifications were also named. The whole series of rocks from the Silurian to the New Red Marl were represented in the low-level Boulder-clay; a few flints also occurred, and one piece of what was believed to be chalk.

The paper concluded with an Appendix by Mr. David Robertson, giving a list of the Foraminifera and other organisms found in the various beds of Boulder-clay in the Atlantic Docks, Liverpool.

2. "On the Evidences of Glacial Action in South Brecknockshire and East Glamorganshire." By T. W. Edgeworth David, Esq. Communicated by Professor J. Prestwich, F.G.S., F.R.S.

The area which is included in this paper is about 200 square miles, extending north and south from the Brecknockshire Beacons to a line between Cowbridge and the mouth of the Rhymney, of which the Cly valley has been more particularly studied. Most of the rocks in this district, and particularly the Millstone Grit, retain traces of glacial markings. The whole area has a *moutonnée* aspect. The evidence of glacial action is classified under the following heads:—(1) erratics; (2) Boulder-clay; (3) shattered and contorted rock-surfaces; (4) grooved and striated rock-surfaces. The first three obtain everywhere; but the last is confined to the coal-basin sandstones in certain localities, to the Millstone Grit at its northern outcrop, and to a small extent of Carboniferous Limestone to the north of the latter.

(1) The erratics consist of Old Red Sandstone, of various members of the Carboniferous series, of dolomitic conglomerate, Lias, and chalk flints. These, in one district, are derived from Brecknockshire rocks, in another from Glamorganshire.

(2) The Boulder-clay contains boulders which are sometimes 5 feet in diameter, generally smoothed, rounded, and striated. It is sometimes 100 feet thick, and is found as high as 1200 feet above the sea. Many sections are described, and percentages of their contents given.

(3) In certain districts the rocks are much shattered, so as to resemble a breccia, and Boulder-clay has been forced into this—as, for example, near St. Fagan.

(4) Grooved and striated surfaces are preserved under favourable circumstances. A full description is given of a number of instances, the direction of the striæ being recorded, as well as the fall in feet per mile from the summit of the Beacons. The author, in summing up his observations, comes to the conclusion that the erratics in the Eglwysilian and Caevan group were probably, as a

rule, transported by floating ice, but that some may be the relics of old moraines, that the Boulder-clay of South Brecknockshire is chiefly the product of land-ice, and that the striated rock-surfaces are in some cases the result of glaciers which have descended existing valleys. In other cases they may have been produced by an ice-sheet, which it is possible may have come from the N.W.

December 6, 1882.—J. W. Hulke, Esq., F.R.S.,
President, in the Chair.

The following communications were read:—

1. Note on a Wealden Fern, *Oleandridium (Tæniopteris) Beyrichii*, Schenk, new to Britain. By John E. H. Peyton, Esq., F.G.S.

2. “On the Mechanics of Glaciers, more especially with relation to their supposed Power of Excavation.” By the Rev. A. Irving, M.A., F.G.S.

1. The author commenced by showing that ice is comparable in some respects to *glass*, with which (at temperatures not far removed from their several points of liquefaction) it has many points of physical resemblance, the chief point of difference between the two bodies being the absence in ice of the great ductility which characterizes glass. Ice may therefore be regarded as a near approximation to the “vitreous condition” of water.

2. The remarkable yielding property (“plasticity,” “*Nachgiebigkeit*”) of ice as it exists in glaciers (which constitutes its most important point of resemblance to heated glass) being recognized as a fact of observation (the experiments of Tyndall and Helmholtz, and the measurements of glacier-movements by the former being referred to), the deduction drawn from these *facts* (irrespective of the theoretical explanation of the facts themselves) in the light of the simple *law of conservation of energy*, is that in the movement of glaciers only a small residuum of “energy of motion” of the glacial mass is available for the work of *erosion*; most of the energy is expended *within the mass of the glacier* in overcoming *cohesion*.

3. If the ice, though *flowing* really in a way comparable to the motion of a river-current (the upper layers moving faster than the lower, the median portions faster than the lateral), retained its *continuity*, the strain against the rocks might be great enough to do much that is required of it by the “erosion-theory;” but here comes in the remarkable *absence of ductility* of ice, giving birth to *crevasses*, the varieties of which are all referable to one common principle, and adverse to erosion.

4. Prof. Tyndall was quoted as an authority for the fact that there is a gradual transfer of ice-particles from the bed towards the surface of the glacier, a fact which the author attempted to explain later on in the paper by reasoning adopted from Helmholtz. The fact itself is directly opposed to erosive action.

5. The instance of the Morteratsch glacier was more particularly considered; and an attempt was made to show that the assumptions which underlie the reasoning by which Prof. Tyndall has endeavoured to meet the objections which have been raised to the erosion-theory from observations of the Morteratsch, are incompatible with sound mechanical principles.

6. The important law of the *lowering of the freezing-point of water by pressure* was next discussed, and reasoning adopted from Helmholtz, which leads to the remarkable conclusion that within the glacier water at 0°C . exists in contact with ice below 0°C . This was accepted by the author as the explanation of the otherwise unintelligible fact referred to in 4.

7. The last point led to the discussion of Dr. Croll's views on glacier-movement. The author gave reasons for rejecting Dr. Croll's so-called "molecular theory" of the movement of glaciers (it is really little more than a restatement of the regelation theory, disguised by a misuse of the terms "molecule" and "molecular"), and for not sharing his feeling of "mystery" about the theory of regelation.

8. A real work of *erosion* was shown to go on in connexion with glaciers, by the direct action of the glacier-streams; but the same objections apply to these as to streams flowing in an open valley, as agents capable of excavating basin-like hollows.

9. The remainder of the paper was mainly occupied with a consideration of "tarns" among the mountains. Here it was admitted that a glacier may work in a different manner from a glacier moving down a valley; and so it was thought many small rock-basins (now tarns) may have been formed at the foot of precipices. On the other hand it was maintained that many tarns occupy hollows formed by earth-movements on the mountain-slopes, or by moraines.

10. In conclusion the author strengthened his position by pointing to the rejection of the erosion-theory by such high authorities as Professors Bonney, Helmholtz, and Credner, and Mr. John Ball, and expressed his regret at finding himself at issue with Sir A. Ramsay, to whose geological writings we all owe so much.

Generally, the author concluded, from mechanical and physical considerations, that far too much *erosive* power has been attributed by some writers to glaciers, and that it is doubtful if the work of actual *excavation* has been accomplished by them at all. The *differential movement* of glaciers he attributed to three causes:—(1) cracking and regelation (Tyndall and Helmholtz); (2) generation of heat by friction within the glacier (Helmholtz); (3) the penetration of the glacier by *luminous solar energy*, the absorption of this by opaque bodies contained in the ice (stones, earth, organic germs, &c.), and the transformation of it in this way into *heat*. To this last he attributed the greater differential movement of the glacier (*a*) by day than by night, (*b*) in summer than in winter.

December 20, 1882.—J. W. Hulke, Esq., F.R.S.,
President, in the Chair.

The following communications were read :—

1. "On Generic Characters in the Order Saurpterygia." By Prof. Owen, C.B., F.R.S., F.G.S., &c.

2. "On the Origin of Valley-Lakes, mainly with reference to the Lakes of the Northern Alps." By the Rev. A. Irving, B.A., B.Sc., F.G.S.

The author, having given reasons for considering this question still an open one, proceeded to criticise Prof. Ramsay's theory as it was expounded by him in 1862. The defects in Prof. Ramsay's argument are, he considers :—(1) the non-recognition of the fact that many lakes in the Northern Alps lie in *longitudinal valleys*; (2) the omission, in the discussion of the relation of valleys to lines of fracture, of the consideration of *anticlinal* lines of fracture, which can be shown to be very common in the Alps; (3) the illogical inference from conditions existing in *crystalline metamorphosed rocks* as to what could or could not occur in the *stratified sedimentary deposits of the Alps, among which the Alpine lakes chiefly occur*; (4) the rejection of the hypothesis of *subsidence* on the mere ground of the number of instances. The author proceeded to show that the lakes of the Northern Alps are found, as a rule, just among those strata where subsidence would be most likely to occur. In this way it was shown that we are *not shut up*, by Prof. Ramsay's reasoning, to the hypothesis of *glacial excavation*.

Further, other agencies than those discussed by Prof. Ramsay may have cooperated to form lakes, such as :—

(a) *Alterations in the relative levels* of different parts of a floor of a valley, connected with movements of parts of a mountain-system on a large scale. The effects of (1) lines of flexure crossing older lines of valley-erosion, (2) of lateral thrusts closing in a valley (partly), were here considered.

(b) *Upthrust* of the more yielding strata (as in the "creeps" of coal-mines) by resolution of forces due to pressure of the mountain-masses at the side of a valley.

(c) The *dead weight of the huge glaciers* which filled the Alpine valleys, and *crushed in the floor*, in places where extensive underground erosion had gone on in preglacial times.

(d) The partial *damming up of valleys*, (1) by *diluvial detritus*, (2) by *moraines*, (3) by *Bergstürze* (recently investigated by Prof. Heim of Zürich).

(e) *Faults*.

(f) *Chemical solution*, by Alpine waters derived from the melting of the snow, which has undergone long exposure to the atmosphere.

It was shown that the very situation of the great majority of the lakes of the Northern Alps is distinctly favourable to the operation of one or more of these agencies. The Königsee was mentioned

as a special instance of *subsidence*, the Achensee of a lake lying in a *faulted* line of dislocation; L. Alleghe and L. Derborence as lakes formed by Bergstürze during the last century; the prehistoric delta of the Arve as the most conspicuous instance in the Alps of the partial damming-up of a valley by diluvial detritus; the *quondam* Lake of Reutte as an instance connected with violent inversion of strata; and the ancient lakes of the Grödner and Oetz Thals as instances of the action of moraines.

The common fact of observation that lakes are more numerous in glaciated than in non-glaciated countries, the author thought, was partly explained by some of the foregoing principles, partly by the better preservation of lake-basins in glaciated countries from silting up and from becoming thus obliterated, while in some glaciated regions lakes are wanting.

XI. *Intelligence and Miscellaneous Articles.*

ON THE EXACTNESS OF THE MEASUREMENTS MADE WITH
MERCURIAL THERMOMETERS. BY J. M. CRAFTS.

A THERMOMETER with a large bulb readily indicates $0^{\circ}002$; but the preciseness of the observation is restricted by slight perturbations caused by variations in the capillary resistance of the stem, by changes of pressure proceeding from barometric changes or owing to the position of the thermometer, and also by calibration-errors and the difficulty of getting a large thermometer to take the temperature of its surroundings.

Neglecting these errors, the sum of which does not exceed $0^{\circ}02$ in suitably arranged experiments, let us examine those which are due to movements of the particles of the glass, slow movements which succeed expansion by heat, and which would involve errors if their effect could not be estimated. Some distinguished physicists have compared glass to sealing-wax: according to them, it yields to pressures, and those of the atmosphere and of the air left in the stem determine the changes of volume of the bulb of a thermometer. Others, estimating at less the part played by pressure, have cited the case of a bar of metal suspended by its extremities, which slowly becomes permanently deformed. These analogies appear to me fallacious: we have not here to do with pressures similar to the enormous force that causes the bending of a bar; we ought rather to compare the bulb of a thermometer to a leaden pipe, which nevertheless supports for years the pressure of a high column of water, whilst the least bending deforms it. Person's experiments, moreover, since confirmed by mine, showed that no important influence could be attributed to pressure.

Numerous determinations tend to prove that we have to do with motions which are but little under the influence of external forces, and that their effects can be foreseen and regulated, by which the precision of thermometric measurements can be considerably augmented.

Depression of the Zero.—Person supposed that thermometers heated for a long time would be rendered incapable of a depression of the zero-point; but that expectation has not been realized: the depressions produced when the thermometer is heated after a long rest cannot be put an end to. The values of these depressions have been determined by several authors; and it has been ascertained that similar experiments give numbers identical to within $0^{\circ}01$ when the thermometers are heated to 100° ; and the errors do not exceed $0^{\circ}04$ for higher temperatures up to 300° . The only precaution necessary is to follow one invariable method of observation: that which appears to me preferable to the rest is to let the thermometer cool in the air and then immediately observe the zero. Some observers, after an experiment at 100° , plunge the thermometer into a bath heated to 50° , and afterwards into cold baths to accelerate the cooling: equally constant results are obtained by this means; but the position of the zero is about $0^{\circ}05$ lower than in the first case. If the thermometer be heated in a large bath holding 20 litres, and the bath and thermometer be left to cool together during twenty-four hours, the zero will be about $0^{\circ}15$ higher than in the first case. If the zero-point be taken before the experiment, or if the experimenter wait some time before observing it, the positions will be higher, but will be more constant if he proceed methodically.

Permanent Elevation of the Zero.—The observations above referred to, as well as all measurements to be made with a mercurial thermometer, are singularly impeded by the permanent elevation of the zero; and the alteration of the coefficient of expansion of the glass which accompanies that phenomenon falsifies all measurements. This movement of the glass-particles varies enormously in its extent, according to the circumstances. Thus in a few hours at 430° , or in a few days at 355° , the zero can be raised 17° or 26° , whilst the writings of M. Libri and later a publication of M. Meucci's establish that, of some thermometers preserved at Florence for more than two centuries, the position of the zero has not notably changed. A fact of especial importance to us is that the permanent elevation of the fixed points, produced at a high temperature, preserves the thermometer from the effect of heat in this respect at lower temperatures. Some thermometers which were kept for eleven days heated to 355° , and were afterwards constantly, during two years and a half, submitted to experiments at all temperatures up to 326° , showed again, after being heated for half an hour to 355° , the same position of the zero, within $0^{\circ}1$, as after the first heating to 355° .

Thermometers intended for our ordinary laboratory experiments should be heated, before graduation and calibration, for a week or ten days in boiling mercury. That is the only appropriate way to obtain instruments that preserve the value of the degree fixed during graduation; and the errors of thermometers which have not undergone this treatment may amount to 4° for a length of 300° .

When the thermometer is intended to give lower temperatures,

it is sufficient to heat it to the highest temperature of the experiments during a very long time relatively to the duration of the subsequent experiments.

Thus a thermometer which indicates the temperature of the atmosphere, and from time to time is raised to 100° in order to fix the value of the degree, is prepared for that use by three or four days' heating to 100° . If, however, it is to serve for prolonged experiments at temperatures near 100° , the whole length of it must be heated to 100° for three or four weeks before graduation and calibration.

If a new thermometer is examined during this treatment, the value of a degree is seen to change in the proportion of about $1:1.0004$; and with the fixity of the zero towards the end of the heating, the value of the degree is observed to have also itself become fixed; and it remains constant if the thermometer is left at the ordinary temperature before a fresh determination of the interval from 100° to zero*.

I agree with M. Pernet in admitting that the value of the degree does not change in ordinary observations, when there is no perceptible change in the position of the zero; but a new thermometer cannot undergo a great number of operations at 100° without one or other of its constants varying. Thanks to the kindness of M. Mascart, I have been able to submit to a long heating to 100° a thermometer which had been in use during more than ten years; and with that instrument no notable change was seen to be produced in the position of the zero. The treatment at 100° does nothing but imitate the effect of long use. The time necessary for the treatment is abridged by heating for twenty-four hours in boiling essence of turpentine, and afterwards from four days to a week to 100° ; and an analogous procedure serves for higher temperatures.

The glass must not be exposed to the corrosive action of boiling water; and metallic apparatus of easy construction permits those operations to be effected without escape and without contact of vapours of either water or mercury.—*Comptes Rendus de l'Académie des Sciences*, Nov. 13, 1882, t. xcv. pp. 910–912.

THEORETIC INTERPRETATION OF THE EFFECT PRODUCED BY A THIN LAYER OF OIL SPREAD OUT AT THE SURFACE OF THE SEA TO CALM THE AGITATION OF THE WAVES. BY M. VAN DER MENSBRUGGHE.

Since the remarkable experiments of Mr. Shields in Scotland, public attention has been called to the marvellous efficacy possessed by oil for calming the surges of the sea. I have the honour of

* It is to be noted that the thing required is to compare zeros depressed to the maximum, and that the depression does not attain its limit at 100° until after an hour or an hour and a half. The time necessary to complete the depression diminishes with the elevation of the temperature.

addressing to the Academy a summary of the propositions with the aid of which I believed I could explain, at the August meeting of the Royal Academy of Belgium, how a small quantity of oil spread out upon a large surface can overcome an enormous quantity of *vis viva* of the waters*.

1. The quantity of work necessary for increasing by 1 square metre the free surface of a mass of water is about 0.0075 kilogramme-metre; this work is stored up, in the form of potential energy, in the fresh surface layer of the water; the thickness of the layer in which that energy resides does not reach $\frac{1}{20000}$ millim.

2. Reciprocally, if the free surface of the water rapidly diminishes, to each square metre of surface lost an energy of motion equivalent to 0.0075 kilogramme-metre corresponds.

3. Let us in thought isolate a mass of water having a base of 1 square metre and a thickness of 1 metre, and imagine that a mechanical action, such as the wind, rapidly rolls up a superficial layer of 1 square metre base and $\frac{1}{20000}$ millim. thickness, laying bare a fresh layer of the same extent; then the potential energy of the first layer will be entirely transformed into energy of motion. If all the layers in succession, each $\frac{1}{20000}$ millim. in thickness, are likewise rolled up, the application of the principle of *vires vivæ* shows that theoretically the cubic metre of water can store up 150,000 kilogramme-metres of work, capable of impressing on the total mass a velocity of 54.2 metres.

4. If a superficial layer of water of 1 square metre surface is caused by the action of the wind to slide over the next layer, of the same extent, the latter, being covered by the former, loses its potential energy, but acquires an equivalent quantity of energy of motion; if the action of the wind makes a fresh layer slide over the first two, there is again developed a *vis viva* equivalent to the lost potential energy of the free surface, and so on.

5. Let us now suppose that a layer of pure water slides over a layer next to it, covered with oil; from that time the potential energy (0.0055 kilog.-m.) of the water covered by a thin greasy layer is replaced by the potential energy of the free surface of pure water (0.0075 kilog.-m.), an energy augmented by those of the two surfaces of contact of the submerged thin layer of oil with the water below and the water above: the value of each of these latter is, according to M. Quincke's measurements, 0.002 kilog.-m. Therefore the sliding of the layer of pure water over the oily layer has produced a *gain* of potential energy of 0.006 kilog.-m. per square metre. But to such a development of potential energy

* It is highly desirable that trials should be made at the mouth of the Seine for the purpose of ascertaining if a relatively minute quantity of oil can, as the facts already known and my theory of the potential energy of liquid surfaces would lead us to presume, prevent the disastrous effects of the violent eddy of the tide. In case of success the employment of oil during storms in harbours, in the vicinity of lighthouses, at dangerous parts of the coasts, &c. would become a duty.

an equivalent loss of *vis viva* necessarily corresponds; and that is the reason why the waves must quickly lose their force as soon as they come into contact with an oily layer.

Such are the very simple propositions which permit me to account for a phenomenon known from ancient times, but which, even on account of its singularity, has not yet obtained the valuable applications it deserves.—*Comptes Rendus de l'Académie des Sciences*, Nov. 27, 1882, t. xcv. pp. 1055, 1056.

ON THE ELECTRIFICATION OF THE AIR. BY M. MASCART.

At one of the meetings of the International Committee for the determination of the electric units, lately assembled at Paris, Sir W. Thomson pointed out the importance to science of continuous observations of the proper electrification of the lower strata of the atmosphere by determining the potential in a limited volume of gas taken from the surrounding air and withdrawn from the action of foreign electrical masses.

I have essayed to see by experiment how a mass of air thus isolated preserves its electrification, in order to define the conditions under which it would be expedient to place one's self for the purpose of continuous observation.

The air of the amphitheatre of the Collège de France, which represents, roughly, a cube of 9 or 10 metres side, was electrified by discharging into it a Leyden jar during 10 seconds by a conducting flame. An electrometer, placed in the room, was in communication with a receiving flame placed about 8 metres from the spot where the discharge took place, and 1.5 metre from the ground. As soon as the discharge is commenced the electrometer is affected: the deflection at first undergoes a series of oscillations of great amplitude, then increases in a more regular manner, reaches a maximum at the end of from 10 to 15 minutes, and afterwards decreases very slowly.

The larger oscillations at the outset appeared to result from a direct action of the electrified strata of air upon the conducting wires of the electrometer, which were too near them. To eliminate this cause of error, the electrometer was placed in an adjacent room, the communication with the receiving flame being established by a wire passing through the partition. In this case the effects are more regular: the maximum deflection was again reached in about a quarter of an hour; it then diminished slowly, obeying a law clearly indicated by the form of the curve to be an exponential, like that for thermal radiation. After two hours the potential was still $\frac{1}{20}$ of its maximum value. Nevertheless there was still manifested, especially during the first minutes, a series of oscillations of small amplitude; and those oscillations were exaggerated as soon as a door was opened even for a very short time, or if an observer crossed the room at several metres distance from the receiving flame.

These phenomena can be naturally explained if it be admitted

that the electrification remains adherent to the strata of air which were directly in contact with the flame during the discharge. The electrified gases ascend by virtue of their raised temperature, then move and disseminate themselves after the manner of smoke until they are uniformly distributed in the atmosphere of the room: the deflection of the galvanometer is then near its maximum. As to the disappearance of the electricity, it takes place either by exchange with the outer air, or by the receiving flame itself continually neutralizing the surrounding electricity, or by contact of the air with the walls of the room.

The loss will be diminished when the movements of gas due to the presence of the flames are suppressed. This is shown by experiment when the discharge is effected by a sharp point and the receiving flame is replaced by running water: the needle of the electrometer is again deflected immediately the discharge commences; but after this it remains some time stationary, attains the maximum deflection a little later, and the return to zero takes place more slowly. At the end of an hour the loss was only two thirds; it would doubtless be much slower in air absolutely still. We have moreover obtained an identical proof of it by studying the air enclosed in a room communicating with the outside only by the usual leakages of the doors and windows: electricity was almost always found there, of the same sign as that of the outer air; for opening a window was sufficient to augment greatly the indications of the instrument.

The electrification produced by a Leyden jar is always very slight; but much more energetic effects can easily be obtained. On the electricity furnished by a Holtz machine being discharged by a flame during one minute, the air was so much electrified that the potential about the middle of the room at the instant of the maximum exceeded 2000 volts; from this the mean density of the electricity in the air can be deduced, supposing its distribution uniform.

From these experiments it follows that, for the examination of the lower strata of the atmosphere, it is sufficient to determine the potential in a room of some metres dimensions, the walls of which should be formed by wire netting with large meshes, in communication with the ground, in order to eliminate the action of external electric masses. The exchanges of gas with the atmosphere, however gentle the wind may be, will be sufficient to compensate the loss produced by the walls and the collecting-apparatus (flame or running water), and to communicate to the electrometer a potential constantly proportional to the electrification belonging to the surrounding air. That potential will be quite different (most frequently of contrary sign) from that obtained by the usual methods. If electricity plays an important part in natural phenomena, it is to be presumed that the proper electrification of the air is peculiarly effective; Sir W. Thomson's suggestion therefore merits the attention of all observers.—*Comptes Rendus de l'Académie des Sciences*, Nov. 13, 1882, t. cxv. pp. 917-919.

THE DISPLACEMENTS AND DEFORMATIONS OF ELECTRIC SPARKS
BY ELECTROSTATIC ACTIONS. BY AUG. RIGHI.

Known experiments show that the electric discharge commences when the electric density on the electrodes has attained a sufficient value relatively to the dimensions of the balls, their quality, distance, &c.* Supposing that the discharge is constituted by the emission of electrified particles, it will commence at that one of the two electrodes on which the density is the greater—from which we derive the explanation of many phenomena.

If, then, at but little distance from the place where the spark is formed there are other electrified bodies, the particles will deviate from their path, moving away from the bodies which have charges of the same name as that of the electrode which repels them, and approaching the bodies charged with the contrary electricity.

Now the spark must pursue the path of the first particles repelled; for, by reason of the heat evolved, it offers less resistance. Therefore the spark itself will deviate, as if it were a body charged with electricity of the same sign as that of the electrode on which the density previous to the discharge is the greater.

The following is one of the ways in which I have verified this class of facts:—The two stems bearing the discharge-balls are arranged vertically one below the other, and at equal distance two parallel vertical disks are kept constantly charged, the one with positive, the other with negative electricity, by a Holtz machine with auxiliary combs, the excitors of which are sufficiently distant from one another for the sparks not to explode. It is readily observed that, when the two disks are not charged, the form of the spark produced between the two stems by the discharge of a condenser charged by another Holtz machine is nearly a straight line (if the distance between the balls is not too great); but if the disks are charged and if the two balls are not in all respects identical, the spark is curved, approaching the one or the other disk. The changes of shape are very remarkable when a liquid resistance such that the spark becomes yellow is inserted in the discharge-circuit. The spark then takes very curious forms; and at the same time it is observed to start from points of the electrodes situated laterally.

Suppose, for example, that the two balls are identical in dimensions and quality, but that one of them, the negative, communicates with the earth; the greater density is then on the positive ball, and it is there that the discharge will commence. The spark, in fact, is displaced and deformed as a positively electrified flexible body would be. The same effect is obtained if, both balls being insulated, the diameter of the negative is greater than that of the positive ball.—*Comptes Rendus de l'Académie des Sciences*, Dec. 11, 1882, t. xcv. p. 1223–24.

* A. Righi, "Sulle scariche elettriche," *Nuovo Cimento*, ser. 2, xvi. pp. 89 & 97.

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XII. *Elementary Investigations relating to Forced Vibrations ; with Applications to the Tides and to Controlled Pendulums.*
By Prof. J. D. EVERETT, F.R.S., Queen's College, Belfast*.

1. **L**ET there be a body whose free vibrations are simple-harmonic, the acceleration for a displacement s being $\mu_1 s$, and the period being therefore $2\pi/\sqrt{\mu_1}$. If this body be acted on by an external force which is a simple-harmonic function of the time, and which urges the body along the same path which it would take if vibrating freely, the body will ultimately be brought into a permanent state of simple-harmonic vibration with the same period as the external force. This is on the supposition (which is practically fulfilled in the majority of actual cases) that, though friction can be neglected in considering a single vibration, its accumulated effect in a great number of vibrations is sufficient ultimately to destroy any previously existing free vibration if the body were left to itself.

2. It is easy to show, by *reductio ad absurdum*, that in the permanent state *the maxima of the external force must coincide with the maxima of displacement*. Let us, for instance, try the supposition that the force attains its maximum after the mean position has been passed but before the extreme position has been attained (and if this be true on one side, it must also, by symmetry, be true on the other). The force in any given

* Communicated by the Author.

position will then be greater in the outward than in the return movement; the work done by the force in the outward movement will therefore not be equal to the work of opposite sign which it does in the return movement; and this difference of work will produce a steady gain or steady loss of energy from one passage through the mean position to the next, which is contrary to the supposition of a permanent state having been attained.

3. The external force then is a maximum in the extreme position; and as this maximum force may either act outwards or inwards, two opposite cases are possible. Either the external force urges the body outward during the whole of the outward movement, and by continuing to act outward opposes the motion during the whole of the return movement; or else it opposes the whole of the outward movement and assists the whole of the return. In this latter case the external force always urges the body *towards* the mean position, and this is precisely what the internal forces (on which free vibration depends) are doing. Moreover, since the displacement, the internal force, and the external force are all three of them simple-harmonic functions of the time, with the same period, and they attain their maxima simultaneously, *the external force has a constant ratio to the internal force.*

4. The acceleration due to the internal force is $\mu_1 s$; the acceleration due to the external force must therefore have the value $\mu_2 s$, where μ_2 is a constant. The actual acceleration will be $(\mu_1 + \mu_2)s$, directed towards the mean position.

This reasoning also applies to the other alternative, except that μ_2 must be regarded as negative, because the acceleration due to the external force will always be *from* the mean position, and therefore opposite in direction to that due to the internal force. In this case μ_2 must be less in absolute magnitude than μ_1 ; for otherwise the actual acceleration would be *from* the mean position—a supposition inconsistent with vibratory movement.

In all the foregoing reasoning, the displacement s may either be a linear distance, or an angular distance, or any other quantity which serves to measure the displacement of a body with one degree of freedom.

5. Denoting the actual period of vibration by T , and the period of free vibration by T_1 , we have

$$T = 2\pi / \sqrt{(\mu_1 + \mu_2)}, \quad T_1 = 2\pi / \sqrt{\mu_1}.$$

If the forced vibrations are quicker than the free vibrations, T is less than T_1 , and $\mu_1 + \mu_2$ must be greater than μ_1 , hence the external force always acts inward; but if the forced vibra-

tions are slower than the free vibrations, $\mu_1 + \mu_2$ must be less than μ_1 , and the external force must always act outward.

6. If α denote the amplitude, or maximum value of s , and f the maximum acceleration due to the external force, which is supposed to be a known quantity, we have

$$f = \mu_2 \alpha,$$

from which equation we can obtain a determination of α in terms of the free and forced periods, as follows:—

$$\mu_1 + \mu_2 : \mu_1 :: T_1 : T^2$$

$$\mu_2 : \mu_1 :: T_1^2 - T^2 : T^2$$

$$\alpha = \frac{f}{\mu_2} = \frac{f}{\mu_1} \frac{T^2}{T_1^2 - T^2}.$$

7. We shall now apply some of the above results to the lunar tides in a uniform canal encircling the earth at the equator, the moon being supposed to be always vertically over the equator, and to revolve round the earth at a constant distance with constant angular velocity.

Let θ denote the difference of longitude of any point of the canal from the point vertically under the moon, and M the moon's attraction upon a unit of mass on the earth multiplied by the ratio of the earth's radius to the moon's distance. Then, as is well known, the moon's disturbing force at any point of the canal can be resolved into a component $M \sin \theta$ perpendicular to the line of centres of the earth and moon, and a component $2M \cos \theta$ parallel to the line of centres—the direction of the former component being *towards* the line of centres, and the direction of the latter component being *from* the medial plane of the earth which separates the hemisphere enlightened by the moon from the other hemisphere. By resolving each of these components along a tangent to the equator, we get $3M \sin \theta \cos \theta$, or $\frac{3}{2} M \sin 2\theta$, as the horizontal force urging the water along the canal, due to the moon's action, its direction being always that which would bring the water nearer to the line of centres. As the value of θ for a given point of the canal increases at the uniform rate of 2π per lunar day, the expression $\frac{3}{2} M \sin 2\theta$ represents a simple-harmonic function of the time with a period T of half a lunar day. As the amplitude of vibration of a particle of the water will be insignificant in comparison with a quadrant of the equator, we may identify the actual disturbing force on a particle with that which would be exercised if the particle were in its mean position, and may therefore adopt $\frac{3}{2} M \sin 2\theta$ as the expression for the disturbing force on a particle.

8. The actual horizontal acceleration of a particle of the water at any moment is the algebraic sum of two horizontal components—one due to the moon and equal (as just shown) to $\frac{3}{2}M \sin 2\theta$, and the other due to the slope of the surface. The magnitude of the latter component is g multiplied by the tangent of the slope; and its direction is always towards the lower side. The particle vibrates under the joint action of these two accelerations, each of which is a simple-harmonic function of the time with a period of half a lunar day. Hence, by the reasoning of section 2, they attain their maxima together. The maxima of the component due to the moon are at the points 45° distant from the line of centres; these must therefore be the points of steepest slope; and as the points of steepest slope in a simple-harmonic wave are midway between crest and trough, there will be either a crest or a trough exactly under the moon. The reasoning of section 5 shows that, *if the actual period be less than the period of free vibration, the two components conspire*; but if greater, they are in opposition. If they conspire, the slope for 45° on each side of the point under the moon must be towards this point; that is to say, there must be low water under the moon. If they are in opposition, the slope must be the other way, and there must be high water under the moon.

The periods of the free and forced vibrations are inversely as the velocities of free and forced waves, since a wave of either kind travels half round the equator in one period. Hence, *if the forced wave travels faster than the free wave, there is low water, and if slower, high water, under the moon**.

9. We can now calculate the height of the tide, if the ratio of the actual period T to the free period T_1 be known.

Let z denote the height of any point of the surface of the water measured from mean level at time t , at a point of the canal distant x in the forward direction from a fixed point of reference, V the actual velocity of the wave, λ the wavelength, which is πr (where r denotes the earth's radius), and $2h$ the difference between high and low water. Then, by the standard formula for simple-harmonic waves, we have

$$z = h \cos \frac{2\pi}{\lambda} (Vt - x) = h \cos \frac{2}{r} (Vt - x),$$

The acceleration due to the slope is

$$-g \frac{dz}{dx} = -g \frac{2h}{r} \sin \frac{2}{r} (Vt - x),$$

* On comparing the reasoning of this section with that employed by Mr. Abbott in a paper on the same subject (Phil. Mag. January 1870), it will be seen that Mr. Abbott has neglected the acceleration due to slope.

the maximum value of which is

$$2gh/r.$$

The maximum value of the component due to the moon is $\frac{3}{2}M$; and the quantities μ_1 and μ_2 in section 5 are proportional to these maximum values. We have therefore

$$T^2 : T_1^2 :: \mu_1 : \mu_1 + \mu_2 :: \frac{2gh}{r} : \frac{2gh}{r} + \frac{3}{2}M,$$

$$T^2 : T_1^2 - T^2 :: \frac{2gh}{r} : \frac{3}{2}M,$$

$$2h = \frac{3}{2} \frac{rM}{g} \frac{T^2}{T_1^2 - T^2}.$$

10. By making T indefinitely great compared with T_1 , we obtain the height of the equilibrium tide, which is therefore (neglecting sign)

$$\frac{3}{2}rM/g.$$

A positive value of $2h$ indicates low, and a negative value high, water under the moon; because we have taken as the standard case that in which μ_2 has the same sign as μ_1 .

Taking the moon's mass as one 80th of the earth's, the moon's distance as 60 times the earth's radius, and this latter as 4000 miles, the value of $\frac{3}{2}rM/g$ comes out exactly 22 inches.

11. The height of the equilibrium tide, and the distribution of elevation and depression over the whole ocean on the equilibrium theory, can be independently calculated by the following simple method, which I have not been able to find in print, though I am informed that it is not new.

Imagine narrow tubes leading down from two points (or any number of points) on the surface to the centre of the earth. The same distribution of water which gives equilibrium when there is freedom for the water to traverse these tubes, will still give equilibrium when they are blocked by diaphragms; for the introduction of diaphragms cannot destroy previously existing equilibrium.

To determine the height of the tide, it suffices to consider two points, one directly under the moon and the other 90° distant.

In the column of water which goes down from the first of these points to the centre of the earth, the moon's disturbing force is vertically upward, and varies as the distance from the centre of the earth. At the top of the column the disturbing force (with the notation of section 7) is $2M$; and its average value for the whole column is therefore M .

In the column at right angles to this, which goes down from the second point, the disturbing force is vertically downward, and has the average value $\frac{1}{2}M$.

As each column has the length r , the pressure to be counterbalanced by difference of levels at the tops of the two columns is $(M + \frac{1}{2}M)r$; and this must be equal to $2hg$, where $2h$ denotes the height of the equilibrium tide. We have therefore

$$2h = \frac{3}{2} \frac{rM}{g},$$

as previously found.

12. The vertical component of the moon's disturbing force at a point of the surface at angular distance θ from the point under the moon, is found by compounding the upward component of a force $2M \cos \theta$ parallel to the line of centres with the downward component of a force $M \sin \theta$ perpendicular to the line of centres, and is therefore an upward force,

$$M(2 \cos^2 \theta - \sin^2 \theta) = M(3 \cos^2 \theta - 1) = M(\frac{1}{2} + \frac{3}{2} \cos 2\theta).$$

The average force through the column which goes down from the point in question to the earth's centre will be half of this, or will be

$$\frac{1}{4} M(1 + 3 \cos 2\theta);$$

and to prevent the column from moving upward we must have an elevation at its summit equal to

$$\frac{1}{4} \frac{rM}{g} (1 + 3 \cos 2\theta).$$

The elevation measured from the mean level of a great circle passing through the point under the moon is

$$\frac{3}{4} \frac{rM}{g} \cos 2\theta,$$

which follows the simple-harmonic law.

13. We have taken no account of the attraction of the elevated water upon the general mass of water. When this effect is included, it makes the height of the equilibrium tide greater by about one eighth part.

14. The principles of sections 1 to 6 have an important bearing on the control of pendulums.

Suppose that the controlling clock gives the controlled pendulum an impulse in one direction at the beginning of every even second of Greenwich time, and an impulse in the opposite direction at the beginning of every odd second. Then, if the controlled pendulum is naturally too slow in its movements, it will adjust itself so as to receive an inward impulse

at each extremity of its swing ; but if it is naturally too quick, it will receive an outward impulse at each extremity. If its natural rate is sometimes too quick and sometimes too slow, according to temperature, it must therefore either gain or lose (practically it would probably always lose) an odd number of seconds when the temperature passes through the critical value.

XIII. *On the Connexion between the Units of Magnetism and Electricity.* By Prof. R. CLAUSIUS*.

IN a paper published in March of this year†, I made some remarks upon the systems of measures to be employed for the measurement of electric and magnetic quantities ; and therein one point occurs which has given occasion to various objections—namely, the determination of the unit of magnetism in the electrostatic system. Some of these objections, which appeared in the June Number of the ‘*Philosophical Magazine*,’ I have already replied to in the August Number of the same journal. Since then, however, some further remarks, proceeding in part from parties worthy of high regard, have been made ; so that I think it necessary again to introduce the subject.

In former times magnetism was regarded as an agent existing apart, independent of electricity. If that view were still adhered to, it would not be necessary, in establishing a system of measures for magnetism, to take into consideration the system employed for electricity. It is, however, Ampère’s great merit, and, I believe I may add, one of the greatest advances made by physics, that an indissoluble connexion between electricity and magnetism has been demonstrated ; so that we no longer need to consider magnetism a separate agent, but can regard all the forces usually called magnetic as electrodynamic.

For if we imagine, in a given magnet of measurable magnitude, each of the innumerable little magnets of which it consists replaced by a small electric current of which the definition is that the product of the current-intensity and the area round which the current flows is equal to the product of the length of the little magnet and the strength of its poles, or, differently expressed, equal to the magnetic momentum of the little magnet, then the totality of these small currents exerts

* Translated from a separate impression, communicated by the Author, from Wiedemann’s *Annalen*, xvii. (1882) pp. 713–719.

† *Verhandl. des naturhist. Vereins der preuss. Rheinlande und Westfalens*, xxxix. p. 105 ; *Phil. Mag.* June 1882, p. 381 ; *Wied. Ann.* xvi. p. 529 (1882).

upon other electric currents or other magnets the same forces as the given magnet. Magnetism can thus, according to Ampère, be accounted for, without assuming the existence of a second agent besides electricity, from the existence of those electric currents.

According to this explanation, by the term Magnetism we are to understand only an electrodynamic conception; and hence, in every system of measures, the unit which is to be employed for measuring magnetism must be chosen in accordance with that conception—that is, so that the product of the unit of magnetism and the unit of length, and the product of unit current and unit area, become equivalent quantities. Therefore, if $[m]$ denotes the unit of magnetism, and $[i]$ the unit of current-intensity, and, further, $[L]$ the unit of length and accordingly $[L^2]$ the unit of area, the following equation must hold:—

$$[mL] = [iL^2]; \quad . \quad . \quad . \quad . \quad . \quad (1)$$

from which follows

$$[m] = [iL]. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Now, since the unit of current-intensity is the intensity of a current in which, in the unit of time, the unit of electricity passes through a cross section, we have, if $[e]$ denotes the unit of electricity and $[T]$ the time-unit, to put

$$[i] = [eT^{-1}];$$

by which the preceding equation is changed into

$$[m] = [eLT^{-1}]. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

This equation expresses the relation between the units of magnetism and electricity corresponding to Ampère's explanation of magnetism. If we apply it to the electrostatic system of measures, and hence define the quantity $[e]$, which in this case must, in order to distinguish it from the electrodynamic unit of electricity, be written $[e_s]$, by the equation

$$[e_s] = [M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1}], \quad . \quad . \quad . \quad . \quad . \quad (4)$$

in which $[M]$ denotes the unit of mass, we get for the definition of the *electrostatic unit of magnetism* the following equation:—

$$[m_s] = [M^{\frac{1}{2}} L^{\frac{5}{2}} T^{-2}]. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

It was this equation that gave rise to the above-mentioned objections, because it differs from Maxwell's, which reads as follows:—

$$[m_s] = [M^{\frac{1}{2}} L^{\frac{1}{2}}]. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The way in which Maxwell arrived at his equation, as may be inferred from the connexion of his analyses, was based on

the employment of an equation which differs from Ampère's with respect to its attitude towards the electrostatic and electrodynamic systems. For a distinction must be made between equations which belong exclusively to *one* of those systems and those which are applicable to *both*. Exclusively electrostatic or electrodynamic equations are those which represent an electrostatic or an electrodynamic force by the formula of mechanical force.

Now, as to Ampère's equation, it certainly resulted from the consideration of electrodynamic forces—that is to say, from the comparison of the forces exerted by a magnet and by an electric current; but it does not represent these forces either by the mechanical-force formula or by any other formula, but merely expresses that the magnet and the current are, in respect of the forces exerted by them, equivalent to one another. It is therefore not to be regarded as an exclusively electrodynamic equation; much rather, since it expresses only the relation which, according to Ampère, subsists between magnetism and electricity, to it must be ascribed, so far as Ampère's theory is at all recognized as correct, a general validity independent of the system of measures employed: and this justifies my application of it.

Maxwell's equation, on the contrary, was produced by representing an electrodynamic force, namely the force which a magnetic pole exerts upon an electric current, by the mechanical-force formula. It consequently has in itself the character of an exclusively electrodynamic equation; and its employment for the determination of the electrostatic unit of magnetism cannot be acknowledged as justified.

On the reasons alleged by various English authors in support of this mode of determination I have already expressed myself in my above-mentioned article in the 'Philosophical Magazine,' and will not repeat what I have there said, but will confine myself to noticing some points which appear to me to furnish a standard for judging the mutually contrary modes of determination.

Maxwell's determination is not only carried out without regard to Ampère's explanation of magnetism, but is even in contradiction to it, as will readily be seen if the formula (6) above given for $[m_s]$, and at the same time formula (4) for $[e_s]$, be inserted in equation (3), because then on the two sides there will be different expressions, and, indeed, expressions of different dimensions. And I must say that, if Ampère's explanation of magnetism, by which the great department of science embracing electricity, magnetism, and electromagnetism is so essentially simplified, were given up again, I should hold that a decidedly retrograde step had been taken.

It is further to be remarked that, if once Ampère's explanation is abandoned and the electrostatic unit of magnetism is sought to be determined in another way, by employing one of the forces exerted by a magnetic pole, then Maxwell's formula is not the only possible one. I am induced to make this remark especially by a memoir recently published by Prof. Helmholtz*.

M. Helmholtz speaks of Gauss's units of electricity and magnetism, which are defined by the following propositions:— (1) The unit of electricity is that quantity of electricity which at unit distance exerts the unit of force upon an equal quantity of electricity; (2) The unit of magnetism is that quantity of magnetism which exerts at unit distance the unit of force upon an equal quantity of magnetism. From these propositions result the equations:—

$$\left. \begin{aligned} [e] &= [M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1}], \\ [m] &= [M^{\frac{1}{2}} L^{\frac{5}{2}} T^{-1}]. \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

A system of measures based upon the simultaneous employment of these two equations M. Helmholtz calls *the hitherto employed electrostatic system*, and in one place also *the electrostatic magnetic system of Gauss*.

Now I certainly do not believe that Gauss intended to make these two equations the foundation of one and the same coherent system of measures; I am much rather of the opinion that he regarded the electric and magnetic systems as two separate and independent systems, of which each would have one of the two equations for its basis. On the other hand, however, I willingly admit that, if we once abandon Ampère's explanation of magnetism, we can just as well combine *these* two equations into *one* system as those which underlie Maxwell's electrostatic system of measures.

We have consequently, including the system recommended by M. Helmholtz, three different electrostatic systems of measures, which agree perfectly with each other in relation to the purely electrical quantities, and differ only with respect to the unit of magnetism; and here, for the sake of distinctness, the three formulæ representing the latter may be placed side by side.

	Maxwell.	Helmholtz.	Clausius.
$[m_s]$	$[M^{\frac{1}{2}} L^{\frac{1}{2}}]$	$[M^{\frac{1}{2}} L^{\frac{3}{2}} T^{-1}]$	$[M^{\frac{1}{2}} L^{\frac{5}{2}} T^{-2}]$

So far as we have to do only with the comparison of mag-

* Wied. *Ann.* xvii. p. 42; Phil. Mag. Dec. 1882, p. 433.

netic quantities with one another, all three formulæ can serve equally well as formulæ for the unit of magnetism ; but if we compare magnetic with electric quantities, the question arises, to what relations between magnetism and electricity do the different formulæ lead ?

In the *electrodynamic* system, between the units of magnetism and electricity the following universally accepted equation holds :—

$$[m_d] = [e_d LT^{-1}]. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

With this we will compare the equations which in the *electrostatic* system have to be formed, according to the three different conceptions of it, between the units of magnetism and electricity.

According to my conception we must put

$$[m_s] = [e_s LT^{-1}]. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

This equation has the same form as that which holds good in the electrodynamic system ; and consequently with this conception we get a definite relation between magnetism and electricity, independent of the system of measures employed. In the equations it is expressed that the unit of magnetism is a quantity having the dimension of a product of the unit of electricity and the unit of velocity. The occurrence of a velocity as a factor expresses that magnetism is to be placed in the same rank, not with resting (static), but with flowing (current) electricity, in accordance with Ampère's explanation.

According to Helmholtz's conception we must put

$$[m_s] = [e_s]. \quad . \quad . \quad . \quad . \quad . \quad (9A)$$

This is quite another equation than that which is valid in the electrodynamic system ; and I must confess I know not what idea is to be formed of the nature of magnetism if in one system of measures the unit of magnetism appears as a quantity having the same dimension as the unit of electricity, and in the other system as a quantity having the dimension of a product of the unit of electricity and the unit of velocity.

According to Maxwell's conception we must put

$$[m_s] = \frac{[e_s]}{[LT^{-1}]} \cdot . \quad . \quad . \quad . \quad . \quad (9B)$$

This is likewise a different equation from that current in the electrodynamic system. Instead of a product of the electricity- and velocity-units, here is a fraction having the electricity-unit for the numerator and the velocity-unit for the denominator; and the question again arises, What idea of the nature of magnetism is to be formed from two expressions so different ?

Bonn, September 1882.

by one tenth ; and in general if a flashes occur in one period, then EUt_2 makes $128 \pm \frac{1}{a}$ vibrations per second.

Since EUt_2 vibrates continuously, the number of periods which may be counted is unlimited; hence a can be found with any desired degree of accuracy.

It was found that the chief difficulty in executing this plan lay in the imperfections of the break-circuit. If thereby the seconds intervals differed as much as 0.002 second, then the operation would be impracticable, on account of the irregularities in the position of the flashes. A great many break-circuits were tried; and the one which was finally adopted as being the most reliable and giving the least trouble was the "mercury globule." Even this, however, in its usual form was unsatisfactory. A great improvement was effected by narrowing the globule in the direction of the swing of the pendulum, by placing it in a small tube whose upper end was flattened. If the current from a "bichromate" cell is interrupted by this break-circuit, the mercury oxidizes rapidly, and the flashes (given by a small induction-coil through the Geissler tube) become irregular.

To obviate this difficulty, the circuit of a "gravity" battery was interrupted; and this worked a relay, which in turn interrupted the circuit of the induction-coil. The results thus obtained left nothing to be desired.

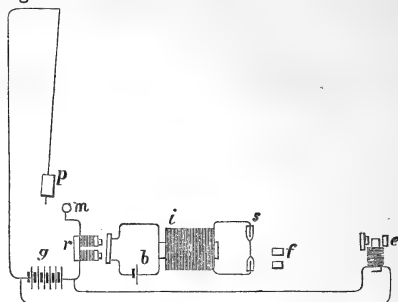
In order that the rate of EUt_2 remain constant, it is necessary that the current passing through its electro-magnets be constant. Hence the necessity for constant batteries, which, however, have a comparatively great resistance. Several might be joined in parallel circuit; but it was found that much better results were obtained by using six "gravity" cells in series, and using fine wire magnets. Such a battery will keep the fork in vibration indefinitely with but little alteration in its rate except that due to variations in temperature.

In comparing the forks a convenient plan is to weight the EUt_2 , so that it gives with the fork to be rated about 9 beats in 10 seconds, and then to time the "coincidences" between the beats and the ticks of the pendulum. By this means the comparison can be much more accurately determined than by counting the beats for sixty seconds. This may also be done by the optical method. It was found that the "gravity" battery which kept the EUt_2 in vibration could serve at the same time to work the relay.

The whole arrangement is shown in fig. 1, in which, for clearness, all the parts to the right of i have been shown in the plane of the drawing, instead of at right angles.

Fig. 1.

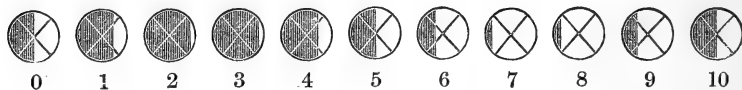
- p*, pendulum.
m, mercury globule.
g, gravity battery.
r, relay.
b, bichromate battery.
i, induction coil.
s, Geissler tube.
f, fork to be rated.
e, "electric" fork.



In practice only the even flashes are counted, the odd ones being disregarded. Consequently if a be the number of these per period, n the rate of the fork, and $2N$ the whole number nearest to $2n$, then $n = N \pm \frac{1}{2a}$. To determine which sign is to be used, EUt_2 is weighted by a very small piece of wax. If a is greater than before, the sign is $+$; if a is less, the sign is $-$.

The electric fork may be dispensed with entirely by the following method. The fork to be rated is placed vertical, so that one edge is in the focus of a microscope provided with cross-hairs. Behind the fork the Geissler tube is placed horizontal. The appearances observed in the microscope at each flash would then be as follows:—

Fig. 2.



From these we can deduce the rate as before.

This method would probably give results almost if not quite as accurate as the preceding, and has the advantage of being more direct. The nearest whole number may be found by comparison with one of König's standards, or by the following method.

The fork is, in turn, compared with two pendulums, whose times of vibration are t_1 and t_2 , $t_1 - t_2$ being small.

Let n_1 = number of vibrations of the fork in time t_1 ;

Let n_2 = number of vibrations of the fork in time t_2 ;

n_1 and n_2 differing from a whole number by less than a small fraction, e_1 .

Let a_1 = number of beats of pendulum (1) per period $= \frac{1}{c_1}$;

Let a_2 = number of beats of pendulum (2) per period $= \frac{1}{c_2}$;
 c_1 and c_2 being small fractions less than e_2 .

Let N_1 = whole number nearest to n_1 ;

Let N_2 = whole number nearest to n_2 .

Then

$$n_1 + c_1 = N_1.$$

$$n_2 + c_2 = N_2.$$

$$n_1 - n_2 + c_1 - c_2 = N_1 - N_2 = M, \text{ a whole number.}$$

$$n_1 - n_2 + c_1 - c_2 \text{ is less than } 2(e_1 + e_2).$$

If, therefore, $2(e_1 + e_2)$ is numerically less than $\frac{1}{2}$, then $M = 0$,
whence $N_1 = N_2 = N$.

$$\frac{n_1}{n_2} = \frac{N - c_1}{N - c_2} = \frac{t_1}{t_2} \text{ and } N = \frac{c_1 t_2 - c_2 t_1}{t_2 - t_1}.$$

Eight determinations of the rate of an Ut_2 fork, made by the preceding method, gave the following results:—

Temp. Fahr.	v. s.	v. s. at 60° F.	Diff. from mean.
54·0	128·134	128·090	+ 0·003
56·3	128·114	128·087	0·000
58·0	128·102	128·087	0·000
60·0	128·090	128·090	+ 0·003
62·2	128·077	128·093	+ 0·006
63·0	128·060	128·082	— 0·005
64·5	128·050	128·083	— 0·004
73·5	127·984	128·084	— 0·003

XV. On a New Form of Ergometer*.

By FREDERICK JOHN SMITH, B.A.†

IN the present period of electrical science, in addition to that branch which deals with lighting, another very important branch has grown out of the great stock, viz. the distribution of power, as the different Companies call it.

In estimating the qualities of various electromotors, and also in certain physical experiments connected with motion (for instance, to illustrate the amount of work done to keep the balls of a centrifugal governor at any given angle), some sort of very sensitive ergometer is required. In dealing with a few foot-pounds per minute instead of many horse-power, great care must be taken that the ergometer be of considerable range, dead-beat in its action, and that it have no internal

* The term "Ergometer" is now logically used for Dynamometer. See Thomson and Tait's 'Elements of Natural Philosophy.'

† Communicated by the Author.

source of error arising from the position of the spring by which the tension of a belt is estimated.

There are several forms of ergometer in which a fast-and-loose pulley are connected together by a spring *b* (fig. 1) or springs, and in which any advance of the loose pulley on the fast one extends the springs. Now as long as the springs are very rigid, such as those used in large ergometers, there is but little tendency to bulge out as they fly round and assume the position shown at *a* (fig. 1). But if the springs are suitable for measuring small quantities, then they are not rigid enough to keep their shape while subject to centrifugal force: such deformation introduces serious error into the total result; it is therefore necessary that the spring be so placed that it is in no way, or but very slightly indeed, affected by centrifugal force. The author finds that this end can be attained if the axis of the spiral be made to coincide with the axis of the moving pulleys. In fig. 2 it will be seen that the spring *A* is placed in a hollow shaft *B C*, part of which *E F* is in the form of a link

Fig. 1.

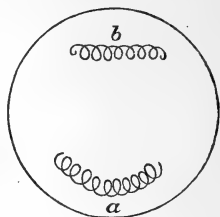
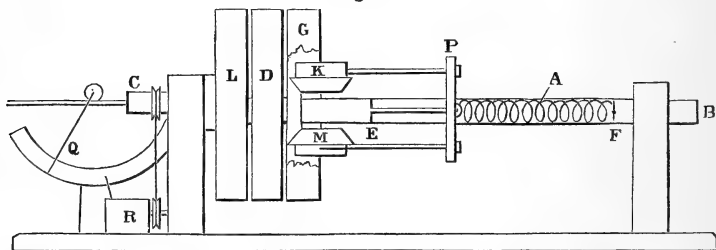


Fig. 2.



to take the spring. This shaft carries three pulleys; of these *L* is loose, *G* fast, and *D* is geared to *G* by means of three mitre-wheels (part of the wheel *G* is supposed to be removed to show these). The mitre-wheels *K* and *M* are provided with flat pulleys, to which the cross bar *P* is attached by two pieces of strong catgut. By this arrangement any advance of *D* upon *G* extends the spring *A*, a light rod connected to the cross bar moves in the tubular shaft, and the projecting end actuates either a pointer *Q*, or an integrating apparatus not shown. With regard to the latter apparatus, the most convenient form perhaps is that in which a diagram of the work done is drawn on a sheet of paper attached to a revolving drum, as used in the ergometer of the late Mr. W. Froude and some others. The area of the diagram can be estimated by the well-known method of

the planimeter; or, if the paper is of great homogeneity, the area may be cut out and estimated by weight (this method is Morin's). The author finds that far the best results are obtained by the use of tinfoil or thin lead instead of paper, as paper is subject to damp and rapid variation of weight. In order that the instrument may be of considerable range, springs of different strengths can be easily interchanged. Since the weight of the indicating rod and bar is slight, the indications are dead-beat. A speed-indicator (this is Young's form of instrument), R, showing the number of revolutions at any instant, is attached to the hollow shaft, and is so placed that its reading and that of the tension-indicator can be seen at a glance. This is found sufficient for most purposes; but when continuous work is being done, such as charging a Faure battery, then the diagram-drum is attached.

For a continuous record, the following form of integrating apparatus has been also employed:—

Hydraulic Integrating Apparatus.

In using the ergometer, it is not always necessary to attach the recording drum, unless some special curve be sought for, such as that which is produced when the instrument is measuring the work done in charging an accumulator. The following apparatus has been devised by the author to take the place of a disk integrator.

A very small double-acting pump, D, oscillating about the point H, and having a long rod, E G, not shown at full length in fig. 3, is actuated by a link A B (rocking about the point B), to which reciprocal motion is given by the connecting-rod, C. This rod receives its motion from the revolution of the shaft; the position of the end of the pump-rod, E, and hence the length of the stroke, is controlled by the rod E F, which is con-

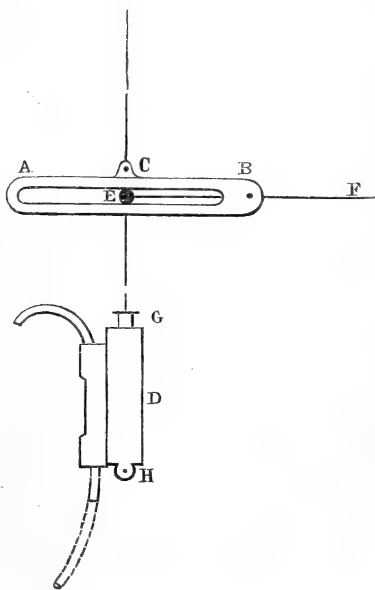


Fig. 3.

nected to the spring of the instrument, so that any extension of the spring moves E towards A, and consequently lengthens the stroke of the pump. Now, when no energy is being transmitted the spring of the ergometer is not extended and E has a definite position between C and B; and then the water discharged by the pump is proportional to the number of revolutions of the shaft; but as soon as energy is transmitted the spring is extended, and the discharge of water is increased by the stroke being thus lengthened: the discharge then becomes a measure of the work done in driving any given machine. Instead of a small pump, a ratchet arrangement was tried; but the results were somewhat inferior to those with the pump. The constants of the ergometer are found in the same manner as when a disk integrator is used.

The Speed-Indicator (second form, by the Author).

This is based on the principle involved in the velocimeter of Ramsbottom. In this instrument oil in a glass cylinder is caused to rotate, the depression caused by rotation being a measure of the number of revolutions. As this instrument does not admit of sufficiently close reading for the ergometer, the author uses mercury instead of oil; this is whirled in a cup with inverted edges. Into the centre of the mercury a fixed tube dips, the end of the tube in the mercury being of twenty times the capacity of the rest of the tube; the tube is partly filled with a tinted liquid; the column of liquid is supported by the mercury. Now, when the mercury is whirled the centre, in sinking, alters the capacity of the large end of the tube; and therefore the indications of the liquid in the small end of the tube are very large for any change in the depression of the mercury. The cup is caused to rotate by means of any two of a set of change-wheels, these wheels being used either to reduce or augment the speed of rotation of the cup as the case may require.

The speed-indicator is attached to the shaft driven by the engine, and not always to the ergometer itself, because in dealing with small amounts of work it is well that the work done in moving the speed-indicator should not be added to the work under test.

The central shaft was made of compressed steel, by Sir J. Whitworth Co.

Taunton, December 19, 1882.

XVI. *The Nature of Solution.* By W. W. J. NICOL, M.A., B.Sc., F.R.S.E., Lecturer on Chemistry, Mason College, Birmingham*.

THE solubility of a salt in water is usually explained by the supposition that it combines with a portion of the water to form a hydrate, more or less stable, and that this hydrate diffuses throughout the mass of the liquid, forming a homogeneous solution. This is more fully stated by Berthelot† as follows:—

“Les phénomènes de la dissolution normale sont en quelque sorte intermédiaires entre le simple mélange et la combinaison véritable. En effet, d’une part, l’aptitude à s’unir pour former un système homogène indique une affinité réelle entre le solide et le dissolvant; mais, d’autre part, cette union cesse sous l’influence d’une simple évaporation, et elle se produit, en apparence du moins, suivant des proportions qui varient d’une manière continue avec la température.

“Cependant il me paraît probable que le point de départ de la dissolution proprement dite réside dans la formation de certaines combinaisons définies entre le dissolvant et le corps dissous. Tels seraient les hydrates définis formés au sein de la liqueur même, entre les sels et l’eau existant dans cette liqueur; hydrates analogues ou identiques aux hydrates définis des mêmes composants, connus sous l’état cristallisé. . . . Je pense en effet qu’il en est ainsi, et que chaque dissolution est réellement formée par le mélange d’une partie du dissolvant libre, avec une partie du corps dissous, combinée au dissolvant suivant la loi des proportions définies. Tantôt cette combinaison se formerait intégralement et d’une façon exclusive; Tantôt, au contraire, cette combinaison ne se formerait qu’en partie, le tout constituant un système dissocié, dans lequel le corps anhydre coexiste avec l’eau et son hydrate, plusieurs hydrates définis d’un même corps dissous, les uns stables, les autres dissociés, pouvant exister à la fois au sein d’une dissolution. Ils constituent alors un système en équilibre, dans lequel les proportions relatives de chaque hydrate varient avec la quantité d’eau, la température, ainsi qu’avec la présence des autres corps etc. Ce serait le degré inégal de cette dissociation des hydrates, variable avec la température, que ferait varier le coefficient de solubilité du corps dissous lui-même.”

I have quoted the above as the most concise and definite statement of the generally received hydrate theory of solution

* Communicated by the Author, having been read before the Royal Society of Edinburgh, January 15, 1883.

† *Mécanique Chimique*, ii. p. 160 *et seq.*

that I have been able to find; but it fails to explain in a satisfactory way many of the phenomena of solution, amongst others the following:—

The alteration in the solubility of salts when simultaneously dissolved in water. The experiments of Karsten* and Kopp† show that in very many cases the solubility of a salt is increased by the presence of a second salt in the solution, while in others, notably in the case of sparingly soluble salts in the presence of more soluble ones, the solubility of the former is diminished while that of the latter is increased.

Again, no explanation is offered of the almost total insolubility of some salts (*e. g.* barium sulphate) and the great solubility of others, nor why the solubility of some salts should increase but slightly with rise of temperature whereas that of others increases rapidly; nor does the explanation admit of extension to such cases of solution as that of glue, where there is no saturation-point, but the solid is soluble in all proportions in water.

Lastly, if the experiments of Wiedemann‡ on the effect of heat on sodium sulphate in the solid state, and my determinations§ of the coefficient of expansion of solutions of that salt, be admitted to have proved that the crystallized salt parts with its water of crystallization at 34° C., whether in solution or not, and, as a consequence, the salt dissolves in water above that temperature in the anhydrous state, we are brought face to face with the difficulty that we have explained solution by hydration and increased solubility by diminished hydration, and at the same time have an example of a salt dissolving in the anhydrous condition, and being then less soluble than in the hydrated state.

The hypothesis I have to offer in place of the foregoing is, I believe, applicable to all cases of solution whether of liquids or solids either in water or other liquids; but the proofs I shall adduce in the present paper are confined to the solution of salts in water. In a paper "On the Condition of Ammonium Salts when dissolved in Water," communicated to the Royal Society of Edinburgh last July, I made the following statement:—

"If we wish to observe the results produced solely by the mutual action of the salt and the water, we must take care that our solutions are sufficiently dilute; as the solution becomes more concentrated the observed specific gravity depends, not only on the attraction of the salt for the water, but also on that of the salt-molecules for one another." This

* *Ann. der Chem. und Pharm.* xl. p. 197.

† *Ibid.* xxxiv. p. 260.

‡ *Annalen der Phys. und Chemie*, 1882, p. 561.

§ *Ber. der deut. chem. Ges.* xv. p. 1931.

gives, I believe, the key to the problem of solution ; it is this mutual attraction of the salt-molecules which, in conjunction with that of the water for the salt, conditions the solubility or insolubility of a salt.

The solution of a salt in water is a consequence of the attraction of the molecules of water for a molecule of salt exceeding the attraction of the molecules of salt for one another. It follows, then, that as the number of dissolved salt-molecules increases, the attraction of the dissimilar molecules is more and more balanced by the attraction of the similar molecules : when these two forces are in equilibrium, saturation takes place. At the saturation-point the force tending to keep in solution any single molecule of salt (attraction of dissimilar molecules) is balanced by the force tending to produce separation of that molecule from the solution (attraction of similar molecules). Further, any external cause tending to alter the intensity of either of these two forces or to modify both in unequal degrees disturbs the condition of equilibrium, and further solution or solidification ensues*.

Before proceeding to a consideration of the experimental results on which the above statement is based, an account of the mode in which my experiments were made is necessary to show the extent to which experimental errors affect the results.

All the determinations of specific gravity of solutions were made in Sprengel tubes ; and these were immersed in a constant-temperature bath at 20° or 40° C., the variation in temperature never exceeding one tenth of a degree. The solutions containing definite quantities of salts were prepared by weighing a quantity of water, calculating the required amount of salt and weighing that directly. The accuracy of this method is shown by the following three determinations of the specific gravity of a solution of sodium chloride containing 2 molecules of salt to 100 molecules of water :—

- | | | | |
|-----|-----------------|---------------|-----------|
| (1) | September 21st, | spec. gravity | = 1.04387 |
| (2) | October 4th, | „ „ | = 1.04393 |
| (3) | October 26th, | „ „ | = 1.04388 |

The solutions were made afresh each time ; and the maximum difference is .00006. But in most cases the error is even less ; for in the experiments on dilution the same solution was diluted at 20° and 40° , thus eliminating error in the weight of salt dissolved ; and thus the limit of error is reduced to that

* In addition to the attraction of water for salt, and of salt for salt, there is that of water for water ; but as that for one temperature and for the cases we are considering is practically a constant quantity, I have not thought it necessary to introduce this third factor in the above account of solution ; we shall see further on what part it plays.

of the variation in the weight of the contents of the Sprengel tubes. This was found in three cases to vary from $\cdot 0002$ to $\cdot 0004$, that is $\cdot 3$ milligramme. A difference of 1 milligramme in the weight of the solution corresponds to $\cdot 00006$ in the specific gravity; consequently the limit of error is $\cdot 00002$, and is generally less than that. No appreciable increase of this quantity is caused by the fact that the weighings were made in air, without correction; for the Sprengel tubes varied but slightly in size, and care was taken to use the same tube throughout each series of experiments: these results then, being comparative, are strictly comparable. The dilutions were made by mixing equal volumes of salt-solution and water. The quantities taken were ten times the specific gravity in grammes and ten grammes of water. The error in volume in no case exceeded $\cdot 0003$ cub. cent. The specific gravities of the solid salts (finely powdered and sifted through muslin) were determined in paraffin by means of a special form of Sprengel tube, thus eliminating errors unavoidable when bottles with movable stoppers are employed. The limit of error is the same as that in determining the specific gravity of the paraffin by means of the same tube. Eleven determinations gave results varying from $\cdot 81831$ to $\cdot 8184$, with a mean of $\cdot 81836$, error $\pm \cdot 00005$. The specific gravity of the solid salt is therefore correct to the fourth place.

Kremers, in 1852*, found that the specific gravity of a solid salt was influenced by the temperature at which it was crystallized from its solution; and that if the solubility of the salt increased with the temperature, the specific gravity of the solid salt diminished with the rise of temperature of the solution from which it separated; if, however, the solubility diminished with the temperature, the specific gravity increased. I have repeated the experiments of Kremers, and the results are contained in Table I.; they agree substantially with his. There

TABLE I.

Salt.	t° .	Sp. gr.	Salt.	t° .	Sp. gr.	Salt.	t° .	Sp. gr.
KNO ₃ ...	20 ^o	2.10355	Na ₂ SO ₄	40 ^o	2.66180	NaCl	20 ^o	2.16171
„ ...	110	2.09916	„	110	2.66372	„	108	2.15494
Difference ... —.00439			+.00192		— .00677	
Water at 20° = 1. t° = temperature of crystallization.								

* Poggendorff's *Annalen*, vol. lxxxv.

can be no doubt of the meaning of this; the specific gravity of a solid is an index of the amount of attraction exercised by its molecules on one another at the moment of solidification (if the determination be made at once); and any further alteration of the extent of the intermolecular spaces can go on but slowly, owing to the exceedingly small internal motion of the molecules of a solid. The separation of a salt from its solution is really a slow process, however quickly (relatively speaking) the solution may be cooled; the molecules consequently are able to arrange themselves in positions of stability for that particular temperature; and once this arrangement has taken place, any further arrangement is, as has been said, extremely slow. Kremers's experiments and mine show that the intermolecular attraction of salts is altered by the temperature, and that this alteration is connected with the solubility: in cases where the solubility *increases*, the attraction is *weakened*; and with sodium sulphate *diminution* in solubility is accompanied by *increase* of attraction.

Further evidence on this point is furnished by Berthelot's recent experiments on the heat of solution of various salts before and after fusion*. When a salt dissolves in water the thermal equilibrium is usually disturbed; in the case of most salts, whether they contain water of crystallization or not, the solution of the form which crystallizes at ordinary temperatures is attended by absorption of heat, due to the heat absorbed by the liquefaction of the salt exceeding the heat evolved by union with the water; or, in other words, the work done in separating the molecules of the salt requires more heat than that given out by the union of water and salt. If the mutual attraction of the salt-molecules be in any way weakened, then less work has to be done to separate them, and consequently the total absorption of heat is diminished. Berthelot finds that in a number of cases the heat absorbed on the solution of a salt in its normal state is greater than that absorbed when the salt has been fused, allowed to cool, and at once dissolved; lastly, if a length of time sufficiently long be allowed to elapse after fusion and before solution, the thermal effects of solution are identical with those observed before fusion. Now a reference to Clarke's tables of specific gravity† shows that in nearly every case where the specific gravity of a substance has been taken before and after fusion, it has been found that the result of fusion has been diminution of specific gravity: but the data existing on this subject are very incomplete and in many instances contradictory, as indeed is the case with the greater

* *Comptes Rendus*, July 1882.

† 'Constants of Nature,' Part I. Smithsonian Institution.

number of specific-gravity determinations. I hope soon to be able to investigate this point more fully.

The next evidence I have to bring forward is that based on the volume-alteration attending the solution of salts in water and the dilution of these solutions at various temperatures; but before doing so it is necessary to have a clear understanding of what is meant by saying that one salt is more soluble in water than another, and that two solutions of different salts are of the same strength. As solution, however it may be explained, is evidently a molecular process, I will in the following pages express the solubility of a salt in water in terms of salt-molecules to a fixed number of water-molecules ($100\text{H}_2\text{O}$), and consider, as indeed my experiments show, that salt-solutions are of the same strength, and are therefore comparable, when they contain an equal number of salt-molecules dissolved in each case in 100 molecules of water.

When a salt is dissolved in water, the volume of the solution is usually less than the sum of the volumes of the salt and the water before solution takes place. This has long been stated as a general rule, to which, among anhydrous salts, ammonium chloride formed the only exception. I have recently shown* that this property is shared by other ammonium salts; and I believe that, on further investigation, it will be found that contraction is by no means the necessary or even the usual concomitant of solution. But in cases where such contraction does occur, it is found that the amount increases as the amount of salt in solution increases, but that the amount of contraction produced by the solution of each successive molecule is less than that produced by the preceding one, as stated by Gerlach†:—

“Die Contraction wächst stetig in allen Fällen, wenn in einer constantbleibenden Wassermenge eine stetig wachsende Anzahl gleicher Salzatome gelöst wird. Die Zunahme der Contraction steht indess keineswegs in einfachem Verhältniss mit der Zunahme gelöster Salzatome; ist letztere eine stets gleiche, so ist erstere eine stetig abnehmende.”

This is shown by Table II., which gives the specific gravity of molecular solutions of sodium chloride, ranging from 1 molecule in 200 of water up to 10.99 molecules in 100 of water (saturated solution).

This is entirely in accordance with my statement of the nature of solution:—the greater the amount of salt already in solution the less the result of the attraction of dissimilar molecules and the greater that of similar molecules.

* Proc. Roy. Soc. Edinb. July 1882.

† *Spec. Gewicht d. Salzlösungen*, 1859, p. 68.

TABLE II.

Water at 20°=1.			
t°.	Strength.	Spec. grav.	Differences.
20°	$\frac{1}{2}$ NaCl	1·01145	$\frac{·02226}{2}$
"	NaCl	1·02258	·02131
"	2NaCl	1·04389	·02048
"	3 "	1·06437	·01972
"	4 "	1·08409	·01857
"	5 "	1·10266	·01833
"	6 "	1·12099	·01739
"	7 "	1·13838	·01665
"	8 "	1·15503	·01637
"	9 "	1·17140	·01567
"	10 "	1·18707	·01484
"	10·99NaCl	1·20191	

This change of volume on solution can also be investigated in another way—the change of volume attending dilution ; and it is this method that is at once the simplest and most accurate. Table III. contains the results of my experiments under this head. In each case a solution of the salt containing approximately 1 molecule to 100 molecules of water was prepared, its specific gravity determined, and then diluted with an equal volume of water.

TABLE III.

Water at 20°=1. C=contraction. Volume before mixing=100,000.								
Salt.	Spec. grav.	1st dilution. Spec. grav.	C.	2nd dilution. Spec. grav.	C.	3rd dilution. Spec. grav.	C.	4th dilution. Spec. grav.
NaCl	1·02252	1·01142	16	1·00571	1			
KCl	1·02542*	1·01283	12	1·00641	0			
NaNO ₃	1·03304	1·01667	15	1·00841	7	1·0042	0	
KNO ₃	1·03379	1·01706	16	1·00857	4	1·00428	0	
KClO ₃	1·04131	1·02088	23	1·01052	8	1·00529	3	1·00265
								0

* This solution is weak. See Table V.

The specific gravity of the diluted solution having been determined, it was in its turn diluted with an equal volume of water. This was repeated till further dilution was unattended by change of volume. With some salts this was found to be the case after the first dilution, whereas with others a second and even a third dilution was required. It may be noted here that, in the case of the less soluble salts, the dilution has to be carried further than with the more soluble ones. I will return to this later on. Now this change of volume on dilution cannot be explained by the hypothesis that, on each dilution, the salt combines with a further quantity of water to form hydrates richer in water; for Guthrie's experiments* show that the cryohydrate is the salt which contains the maximum number of water-molecules with which the salt can combine; and, as shown in Table IV., the solution which gives no change of volume on dilution contains far more water than that required by the corresponding cryohydrate; in addition, the temperature 20° C. is in every case much higher than the temperature of formation of the cryohydrate, consequently the disproportion of water to salt is much greater.

TABLE IV.

Salt.	Approximate strength. (Table III.).	Cryohydrate.
NaCl	1 molecule to 200 H_2O	1 molecule 10.5 H_2O
KCl	1 " " 200 H_2O	1 " 16.6 H_2O
NaNO_3	1 " " 400 H_2O	1 " 8.13 H_2O
KNO_3	1 " " 400 H_2O	1 " 44.6 H_2O
KClO_3	1 " " 800 H_2O	1 " 222 H_2O

Berthelot's experiments† are equally conclusive. He found that the heat of solution of a salt varied with the temperature, that the higher the temperature the greater the amount of heat evolved. This appears quite irreconcilable with the hypothesis that the salt combines with less water at a high temperature than at a low one, while it is in accordance with the results of my experiments on the molecular attraction of salts at various temperatures. Further, Table V., which embodies my experiments on the effect of dilution of salt-solutions at 20° and 40° , shows that the effect of a rise in temperature is to lessen

* Phil. Mag. [4] xlix. p. 1 &c.

† *Annales de Chimie et de Physique*, [5] 1875, iv. p. 28.

the contraction on dilution in the case of salts whose solubility increases greatly with the temperature, while it scarcely affects those salts whose solubility remains nearly constant.

TABLE V.

Salt.	t° .	Spec. grav.	Spec. grav. after dilution.	Mean spec. grav.	C.	Solubility.
NaCl	20 ^o	1.02257	1.01147	1.01128	17+	10.99 mol.
	40	1.02182	1.01103	1.0109	13	11+ "
KCl.....	20	1.02569	1.01301	1.01284	17	8+ "
	40	1.02516	1.01272	1.01258	14	9+ "
NaNO ₃ ...	20	1.03161	1.01596	1.0158	15+	10+ "
	40	1.02998	1.01509	1.01499	10	18+ "
KNO ₃	20	1.03352	1.01692	1.01676	16	5+ "
	40	1.03232	1.01619	1.01616	3	10+ "
KClO ₃ ...	20	1.04131	1.02088	1.02065	23	1+ "
	40	1.04014	1.02024	1.02007	17	(2- ?) "
Solubility x molecules in 100 H ₂ O.						

Again, the determinations of the coefficient of expansion of salt-solutions lend additional support to this view of their constitution. Gerlach* and Kremers† have, with others, done a large amount of work in this direction; and the result appears to be that, for weak solutions of salts, the coefficient of expansion is in most cases greater than that of water for the whole range of temperature from 4° to 100° C.; and this is also the case with strong solutions up to a certain temperature, which varies with the salt and with the degree of concentration, but is lower the more concentrated the solution. These phenomena may, I think, be satisfactorily explained as follows, according to the statement on page 93. We have three factors to consider in a solution:—

The attraction of water for water . . . = x ,

That of water for salt = y ,

That of salt for salt = z .

Now, in a solution the effect of heat on x and y will be to produce expansion, and on z to produce contraction; for if z be weakened y is increased. If z be very small, as in a weak solution, it will scarcely affect y ; and the result will be that, in addition to the expansion of water, there is the expansion due to the effect of heat on y . If, however, z be very large, as in a strong solution, y is correspondingly smaller in proportion; and a weakening of z is accompanied by a large

* *Spec. Gewicht der Salzlösungen*, Part III.

† Poggendorff's *Annalen*, vol. c. &c.

increase of y —it may be, more than equal to the weakening due to heat; consequently contraction ensues, tending to decrease the expansion due to the water.

Table VI. is partly taken from Kremers's paper, and will show that the above is the case.

Volume at $20^{\circ} = 100,000$.

Δ = difference between the volume of the solution and that of water at the same temperature.

The strengths are respectively 10 and 40 "salt atoms" to 100 parts by weight of water (see Poggendorff's *Annalen*, xcv., xcvi., xcvi., &c.).

TABLE VI.

KCl.				
t° .	Vol. 10 s. a.	Δ .	Vol. 40 s. a.	Δ .
20°	100,000	0	100,000	0
40	100,700	+107	100,807	+214*
60	101,626	+110*	101,727	+211
80	102,767	+ 60	102,782	+ 87
100	104,134	+ 5	103,954	-175
NaCl.				
20	100,000	0	100,000	0
40	100,730	+137	100,895	+302
60	101,673	+157*	101,880	+364*
80	102,826	+125	102,983	+282
100	104,187	+ 58	104,213	+ 84
LiCl.				
20	100,000	0	100,000	0
40	100,613	+ 20*	100,592	- 1
60	101,480	- 34	101,327	-189
80	102,565	-136	102,206	-495
100	103,866	-263	103,220	-909

* Point of maximum expansion.

In conclusion I may point out that, in cases where the same substance exists in more than one form, the variety that is most soluble or is most readily attacked by acids is the one with the least specific gravity. Sulphur forms, I believe, the solitary exception to this rule, which is followed by phosphorus, arsenic, arsenious oxide, selenium, carbon, and also by that class of compounds of which ferric and aluminic

oxides are well-known examples, the specific gravity of which increases with the amount of heat to which they are subjected, while their solubility in acids diminishes.

I believe, then, that it will be possible to connect together the molecular volume and the solubility of solids, just as the molecular volume and the boiling-point of liquids have been shown to be connected. The problem is necessarily a more complex one, as we have to deal not only with the mutual attraction of the molecules of the solid, but also with that of the water for the solid, which varies not only with the temperature, but also with the nature of the solid. However, some experiments I have already made lead me to believe that such determinations are possible.

Since the above paper was written, my attention has been drawn to an article on Solution by Dossios*, which I had known only in abstract: our theories have much in common; but Dossios's is unsupported by experimental evidence.

XVII. On the Radiometer. By ERNST PRINGSHEIM †.

THE uncertainty and differences of opinion which still exist respecting the cause of the motion in the radiometer and similar apparatus appear to arise from the circumstance that in fact the motion of these instruments is essentially influenced by several different things, the actions of which are reciprocally so complementary and disturbing that by the complexity of the different results the conformity to law which prevails among them is often obscured.

If we ask what are the things which may have influence upon radiometer-motion, they are, first, the different parts of the apparatus itself—namely the glass case, the enclosed gas, and the vanes. Therefore the only right way to study the mode of working of radiometric apparatus will be, to separate as much as possible the effects of these different parts experimentally, and to treat each of them singly.

APPARATUS.

For this purpose the ordinary radiometers are extremely unsuitable, as in them the phenomena are very much complicated by the moving force always appearing simultaneously

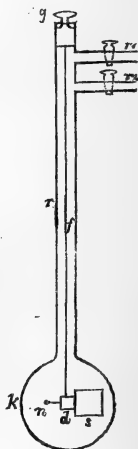
* "Zur Theorie der Lösungen," *Vierteljahrsschrift der zürichischen Naturforschenden Gesellschaft*, xiii. pp. 1-21; *Jahresbericht*, 1867, pp. 92-95.

† Translated from Wiedemann's *Annalen*, 1883, No. 1, pp. 1-32.

at several vanes, so that any possible opposed actions of two vanes must first be ascertained by special experiments; while it is very difficult really to determine what portion of the motion proceeds from one vane alone, since this is frequently disturbed by reflections of light, which, especially when the action of bright surfaces blackened at the back, with radiation of the bright side, is to be ascertained, are often apt to reverse the entire motion. Besides, the vanes in their rotation change their position with respect to the source of heat, and therefore with the essential conditions of the motion, so quickly, that perhaps dissimilar phenomena, separate in time, occurring at one vane, are completely obliterated and rendered undistinguishable. And, lastly, a relatively strong force is requisite in order to overcome the resistance presented by friction and set the radiometer rotating; so that it is a comparatively insensitive apparatus.

These inconveniences are avoided when, instead of a radiometer, a so-called torsion-apparatus (like several constructed by Crookes) is employed. In these we have, as a rule, to do (unless the differential actions of several faces are to be observed) with one vane only, which is suspended by a thread and, on receiving the radiation, is deflected a certain angle, till the torsion of the thread counterbalances the effective force. Such an apparatus, but of more simple construction than most of Crookes's, I constructed for myself; and with it I carried out the experiments particularly described below.

It consisted of a glass tube r , 435 millim. in length and 22 millim. in diameter, blown out at one end into a spherical bulb, k , of 80 millim. diameter; into the other end a ground glass stopper fitted air-tight. To a small hook projecting from the stopper a very fine glass thread, f , 420 millim. long, was cemented, to the other extremity of which a small, very thin microscope covering-glass, d , silvered behind, was fastened, to serve as a mirror. To the back of this mirror one extremity of the rectangular disk s , of 24 millim. length and 16 millim. width, serving as a vane, was cemented with hot wax, while on the other side the disk was counterpoised by a pin, n . The whole, attached to the glass thread, was slowly lowered through the glass tube; and then the stopper, previously greased, was inserted, air-tight, so that the mirror hung in the centre of the bulb.



From the glass tube two lateral branch tubes, r_1 and r_2 ,

furnished with glass cocks, proceeded, by which the apparatus could be connected with the air-pump and a gasometer. To produce a vacuum in the apparatus a Geissler mercury air-pump was employed ; and the exhaustion was always carried on till the volume of air expelled by the rise of the mercury-level ceased to diminish, in spite of continued pumping. This pump being employed, the finally remaining pressure could not be determined, because the pump possessed only an ordinary mercury manometer ; in this, however, on exhausting, the mercury stood somewhat higher in the open than in the closed leg ; so that the rarefaction was at all events considerable, and, as may be inferred from the sensitiveness of the apparatus, not far from the pressure at which the mobility reaches its maximum. Moreover, an exact knowledge of the amount of pressure is immaterial in our experiments.

For the experiments the instrument was fixed, as perpendicular as possible, on a tripod, so that the vane could rotate with perfect freedom. The reading-off of its position was secured by the following arrangement:—The image of a fine slit illuminated by a petroleum lamp was concentrated upon the mirror by a lens, and reflected thence to a scale divided into millimetres at about 1100 millim. distance. In order, however, to be quite safe from any disturbing action of this light, the latter should first be caused to pass through a layer of water and solution of alum, and thus deprived of the rays which are effective upon the apparatus.

To protect the whole from air currents it might be put into a tin case stuffed with wadding, having only two apertures to permit the luminous index and the rays of the source of heat to fall upon the mirror and the disk respectively.

This apparatus permitted a relatively convenient and quick changing of the disks, although this is a sufficiently delicate manipulation; and in lowering in and taking out the apparatus more than one glass thread broke, since the disk with the mirror could only slide through the narrow glass tube lengthways, so that the thread had to bend sharp round directly on the mirror. This inconvenience could have been remedied only by inserting the apparatus in a proportionally wide cylindrical tube ; but then the space to be exhausted would have been too large, or the thread must have been short, and hence the sensitiveness too little ; moreover the desired similarity of the glass case in the vicinity of the vane to the globe of the radiometer would have been lost.

In most of the experiments the cock of one of the side tubes was closed, while the other tube was connected with the air-pump by very short india-rubber connexions and a long

elastic glass tube. The cocks and stopper were all well greased, which enabled me, with a little attention, to keep the apparatus completely exhausted for days together without any perceptible increase of the air-pressure.

I. Action of the Glass Case.

On the direct action of the glass case of the radiometer no prior experiments are known to me. It is true that experiments were made by Crookes * for the purpose of ascertaining what influence is exerted by a greater or less distance of the vanes from the glass sides, but only in order to show that the motion depends on a reciprocal action between the vanes and the case.

First, the question presents itself, Is the glass case, in itself, an indispensably necessary condition for the generation of the motion of the radiometer?—that is, would the motion cease, or be different in quality, if the movable part of the apparatus were suspended in an unlimited space of rarefied air? Crookes once affirms this †, emphatically stating that in the radiometer, in contrariety to the otheoscope, the glass case is necessary for the motion, but without mentioning his reasons either in the place cited or, to my knowledge, anywhere else. He was perhaps led to this conclusion by theoretical considerations, which he indicates in another place ‡, where he adopts Stoney's theory.

To decide this question directly by a simple experiment is, of course, impossible, since we cannot produce even a rough approximation to an infinite space of air so highly rarefied. Hence the decision of this point is only possible by approaching it indirectly, and probably not without theoretical discussions; therefore we have not yet come to the place for entering more particularly into it.

A supposition, however, which certainly at first obtrudes itself respecting the behaviour of the glass case, is very easily refuted. For a very simple observation shows that in the ordinary radiometers, consisting of thin mica vanes blackened on one side with soot, irradiation of the blackened side alone with sunlight or an ordinary artificial source of light produces rotation in the same direction as irradiation of the bright side only, so that in both cases the black surface recedes. Now, on the assumption that the rotation is produced by the difference of temperature which actually

* *Comptes Rendus*, lxxxiii. p. 1233 (1876); *Proc. Roy. Soc.* xxv. pp. 308, 309 (1876).

† *Comptes Rendus*, lxxxiv. p. 1081 (1877).

‡ *Proc. Roy. Soc.* xxv. p. 308 (1876); *Nature*, xv. p. 224 (1877).

occurs between the glass case and the vanes, it might easily be believed that the temperature of the former lies between the temperatures of the bright and the dark surface, and therefore exerts an opposite influence on both. But the glass case can be kept at the temperature originally possessed by the entire apparatus by inserting in the path of the rays a layer of plane glass plates of such thickness that all the rays generally absorbable by glass are kept back in them; and in this way it is easy to ascertain that even then a sufficiently powerful source of light produces the same effect, although in a less degree, as without the interposition of the glass plates. Further, that all the rays absorbable by glass are actually absorbed by the glass plates before reaching the apparatus, can with facility be known from this—that the insertion of additional glass plates between the source of light and the instrument has no effect upon the motion. This proves, therefore, that the above assumption does not correspond to the real cause of the opposite action upon the bright and the dark side of the vanes, and consequently furnishes a confirmation of the view enunciated by Cooke *, that the rays which have passed through the mica, being absorbed by the soot, produce sufficient heating to cause the motion.

Now, although it is not possible, as above remarked, to decide by experiment whether the glass case is absolutely necessary for the production of the motion, yet we can get some elucidation of its actual operation. For as all the rays incident upon the vanes must first pass through the glass, it is evident that it must absorb a portion of the radiation and thereby have its temperature raised. Probably it is now possible to ascertain what influence this rise of temperature of the glass sides alone would have if the vanes themselves retained their initial temperature during the entire experiment, or at least showed no difference of temperature on their two sides.

To effect this, in the above described torsion-apparatus a mica plate as thin as possible was employed as a vane, from which it was to be presumed that it would be capable of absorbing very few rays indeed, and that, with its extraordinary thinness, both sides would always have the same temperature. When now our apparatus, suspended in the open air without its tin case, was irradiated, the side of the glass case which was turned to the source of light must have absorbed more rays than that which was turned from it, while it could be assumed that the mica plate possessed the same temperature

* Silliman's Journal, xiv. p. 237 (1877).

on both sides. The light-source was placed so that its centre was in the normal to the centre of the mirror ; so that the cone of rays issuing from it was tangent to the glass sphere in the same vertical plane in which the mica vane hung. Therefore, if our assumptions are correct (which shall subsequently be still more closely investigated), and if it be further assumed that any possible absorption within the rarefied air is without influence (a question which will also be afterwards discussed), any effect that may be produced must be ascribed to the heating of the glass side alone. .

First some experiments were made with sunlight reflected from the metallic mirror of a Duboscq heliostat upon the apparatus. With these there was not the slightest motion of the spot of light.

The thing took another shape, however, when the flame of a Bunsen burner was employed as the source of heat. There were now energetic deflections which were all positive—that is, in which the irradiated side recedes—amounting, on the average, to 65 millim.

We see therefore that repulsion of the vane by the more strongly heated side of the glass case takes place, if our above suppositions are correct.

But now it might still be possible that we should have to distinguish in the thin mica plates a front and a back side, and that the front face absorbs a greater portion of the rays than the back, hence becomes warmer, and, in consequence of this, recedes.

In order to test this possibility the glass case was irradiated laterally and at the same time half of it shaded so that the mica plate was in the shade, and its plane formed exactly the plane of division of light and shade. If now motion again occurred, it certainly could not arise from direct absorption by the mica plate, since this itself was outside of the cone of light.

On employing sunlight, again no trace of motion appeared; and consequently, as directly follows from the action of the gas-flames, it was proved that the thin glass absorbed only vanishingly little of the sun's rays.

Lateral irradiation by a gas-flame, on the contrary, effected a deflection in the same direction as before—namely, so that the mica plate was always repelled from the illuminated side of the glass.

The results were :—

I. With lateral irradiation from the vane side (so that the part of the glass case nearest to the vane was first struck by the light) :—

Resting-position of the Spot of Light : On the Illumination

	Of the fore half.	Of the entire sphere.	Of the hind half.
Bright gas-flame ...	468	500	531
Deflection ...	32	31	
Dull gas-flame ...	447	500	552
Deflection ...	53	52	

II. With lateral irradiation from the side situated furthest from the mica plate :—

Resting-position of the Spot of Light : On the Illumination

	Of the fore half.	Of the entire sphere.	Of the hind half.
Bright gas-flame ...	496	500	504
Deflection ...	4	4	
Dull gas-flame ...	493	500	507
Deflection ...	7	7	

These experiments therefore show that in fact the warmed glass side exerts a repellent force upon the vane ; and at the same time it follows, from the difference of the effects when the irradiation proceeded from the side furthest from and from that nearest to the mica plate, that the parts of the glass, situated nearest to the vane almost alone exert the action. This remark will be corroborated by the following experiment.

The apparatus was illuminated, as in the first-cited experiments, by means of a bright Bunsen burner ; and then the greater part of the light-cone was so far cut off that only the outermost part of the glass, nearest the mica vein, was irradiated, while the vane itself was in shadow. The following was the resting-position of the spot of light :—

With full illumination.	Without illumination.	With partial illumination.
465	500	471.
Deflection 35		29

Almost the whole of the effect therefore comes from the part of the glass side immediately opposite to the mica vane.

After the above experiments with lateral illumination it cannot any longer be doubtful that the deflection is really owing to the heating of the glass side ; but from this it does not yet appear whether this effect is a direct repulsion brought about by the air particles present between the two surfaces,

or a secondary one produced by the heat radiated from the heated glass side being more strongly absorbed by the front than by the back surface of the mica. The latter, however, is in the highest degree improbable; for certainly a great part of the heat conveyed into the glass by absorption is communicated to the colder air in the interior of the apparatus; and of that withdrawn from the glass by radiation, only an extremely small part can pass over to the mica plate, since it is radiated in the space uniformly in all directions. Hence the proportionally strong action is scarcely to be attributed to this minute quantity of heat, but, at any rate for the most part, proceeds from the heat communicated to the gas by conduction.

The mica, too, would certainly absorb a portion of those rays which come to it direct from the source of heat, which must have the same action as those emitted from the glass; so that with the irradiation first employed, from the front, the two actions would be added together, while with the lateral irradiation only the action of the glass is present. Therefore, the mica plate being at equal distance from the same source of heat, the deflection ought to be greater in the first case than in the second. But it is not so, as the following observations show:—

Distance of the bright gas-flame from the outermost perpendicular edge of the mica plate, 305 millim.

Resting-position of the Spot of Light.

I. With irradiation from the front :

Without illumination.	With illumination.	Deflection.
500	465	35

II. With lateral irradiation :

With illumination of the whole.	With illumination of one half.	Deflection.
500	447	53

The deflection in the second case is therefore not only not weaker, but considerably stronger than in the first—which probably results from the circumstance that with irradiation from the front the side of the glass facing the back side of the mica plate is heated by absorption, and thus counteracts the action of the front half of the glass, while with the irradiation lateral only the favourably acting half of the sphere is heated.

Besides these probable reasons, a strict proof by experiment presents itself that the repellent action of the heated glass side is a primary and not a secondary action. If, namely, it proceeded from the absorption by the vane of the rays emanating from the glass, its amount would depend on the absorptive capacity of the vane, and consequently on its substance.

In order to put this dependence to the proof, the transparent mica vane employed in the previous experiments was blackened on one side with soot and again inserted in the apparatus. The source of light was placed at the side of the apparatus (as in the experiments described in pp. 106 and 107), in the plane of the vane, so that the part of the glass case in front of the vane was as strongly illuminated as the half situated behind it.

If the action depends on the substance of the vane, with this arrangement the irradiation of one half of the glass sphere, while the other half and the vane are shaded, ought to produce a quantitatively different deflection from that produced by the irradiation of the other half. On the other hand, if the action consists of a direct repulsion, it is evident that it will remain unaltered whether it strikes a soot or a mica surface, and then the deflections produced by both halves of the glass case must be of the same magnitude. The experiment confirmed the latter supposition. At the same time it should not be forgotten that all other influences, especially those of neighbouring objects and the walls of the apartment, must be carefully kept off from the apparatus, as in this form it is very sensitive, and the arrangement of the experiment makes it impossible to surround it with a protecting case.

As with lateral irradiation of the entire glass case direct heating of the soot is evidently unavoidable, in the following Table the deflections are reckoned from the resting-position when the entire apparatus is completely shaded.

The experiments, conducted with the requisite precautions, gave the following result:—

Resting-position of the Spot of Light on Shading

	The blackened side of the glass.	The entire glass case.	The mica-side of the glass.
Bright gas-flame ...	545	500	455
Deflection ...	45		45
Bright gas-flame ...	576	500	425
Deflection ...	76		75
Dull gas-flame ...	555	500	445
Deflection ...	55		55
Dull gas-flame ...	570	500	430
	70		70

That this effect indeed proceeds only from the glass case is known from the fact that even a plane glass plate of 8 millim. thickness, placed between the source of light and the apparatus, is sufficient to prevent any deflection.

As the deflection of the translucent mica vane when irradiation takes place from the front arises from the excess of the amount of heat absorbed by the fore half of the glass case over that absorbed in the hinder half, it will be possible to reverse the motion by so arranging the glass case that its hinder half absorbs more heat than the fore half. This can be done by blackening the hinder half with lampblack. But when this was done and the apparatus irradiated by a bright Bunsen burner, there appeared at first not a reversal, but only a weakening of the deflection, and only by inserting glass in the path of the rays could a reversal be obtained. Consequently with full irradiation the heating of the clean half of the glass was stronger than that of the blackened half, and it was only when the rays most absorbable by glass were removed before reaching the sphere that the absorption in the lamp-black gained the preponderance. In this experiment the resting-position of the spot of light was :—

Without illumination.	With full illumination.	With light transmitted through 15 millim. thick- ness of glass.
500	489	554
Deflection ... 11		54

It was different when sunlight was employed, which had the greatest effect without any intervening glass, and produced a deflection of 50 millim.—a fresh proof that extremely little of solar radiation is absorbed by glass.

All the above experiments prove that the heated glass side exerts upon a movable surface suspended in front of it a repellent force which is independent of the substance of the surface—or, differently expressed, that a pressure emanates from the heated glass side which increases with the temperature of the glass.

In numerous similar experiments it has by previous observers been ascertained that any warmed surface tends to repel a light body suspended over against it in a space of rarefied air.

Therefore, having now proved that this repulsion is a direct one, and not generated by absorption of the heat radiated by the warmed body, we can express the above proposition in the following amplified form :—

From a warm surface, in a space containing rarefied air,

emanates a pressure which increases with the temperature of the surface.

We have (p. 106) made the supposition that in our experiments absorption of heat within the rarefied air filling the radiometer has no influence upon the motion. Since, however, on lateral irradiation this air is illuminated as well as the glass side, it might still be possible to refer the generated motion to a rise of temperature of the residual air, produced by absorption. That this would be erroneous we will demonstrate in the following section.

II. *Action of the Residual Gas.*

It must first of all be premised that, according to all theories of the radiometer that rest on the kinetic theory of gases, any one-sided conveyance of heat to the gas must cause a motion of the vane; and therefore a local rise of temperature produced by absorption in the rarefied air must do so also. Yet, at the high degree of rarefaction at which the air in the radiometer is, it is in itself very unlikely that that air is capable of absorbing any considerable amount of heat, especially as the rays from the source of heat must, before reaching the apparatus, pass through so great a thickness of atmospheric air that they are doubtless completely cleared of all the rays which can be absorbed by air.

In relation to this I made some experiments with sunlight and gas-light, by throwing close before the translucent mica disk of my apparatus, arranged as in the experiments described above, a focus in the rarefied air.

In order to avoid the influence of the heating of the glass side, I caused the light to fall from above obliquely into the apparatus, so that the glass was struck by the light at a place obliquely above the mica plate at a greater distance from it. There was then not the slightest perceptible deflection of the vane. This proved that in the above experiments an absorption of heat by the rarefied air had no influence on the result.

Yet, in order to prove by experiment the theoretically important assumption that absorption of heat by the gas might produce motion of the vane, I filled my apparatus with illuminating-gas instead of atmospheric air, by connecting the second side tube with a drying-apparatus which was connected with the gas-pipe supplying the house. After filling and exhausting the apparatus five times, so as to be certain that I really had in it rarefied illuminating-gas, I repeated the absorption-experiments which had been made with atmospheric air, but with the same negative result. It appears, therefore,

that in gas so highly rarefied the absorption is vanishingly little. In other respects the apparatus showed the same phenomena with illuminating-gas as when atmospheric air was employed.

III. Action of the Vanes.

By far the greatest influence on the manner of motion of radiometric apparatus is exerted by the quality of the vanes themselves. First of all, it is evident that motion can only be produced by the vanes when on irradiation their two sides act differently; for one and the same action of both sides would neutralize itself. This different action presupposes a difference in the quality of the two sides, which difference may lie in their consisting of different substances, or having different forms, or being of the same substance but having different temperatures.

As to the first point, the great complexity of the cooperating properties of the bodies renders it hardly possible to state exactly the influence exerted by each property of the substances used as vanes.

In the first place, the forces which generally come into play are very slight; hence it will be necessary to employ extremely light substances—such as pith, mica, or very thin metal plates.

Further, as the motion is produced by irradiation, it is clear that the *vis viva* of the motion is derived from the rays. But this is only possible by absorption, since even the simplest experiments show that the cause of the motion cannot be a direct transference of the *vis viva* of the rays to the vanes. Hence the greater the amount of rays absorbed in the vane, and the more the portion of the radiation absorbed by one surface exceeds that absorbed by the other, the stronger will the action be. To this inference correspond also the observations* which show that the action is strongest when thin mica plates, which absorb almost no rays at all, are coated on one side with lampblack, the blackest of all known substances also for heat. Less favourable are metal plates blackened on one side with lampblack, since metal radiometers on being radiated upon by dark sources of heat move with the lampblack side in front, therefore inversely as in light†. From this it is to be inferred that the metals are still more impenetrable to heat-rays of very great wave-length than soot—which also agrees with the experience that soot in thin

* Crookes, Phil. Trans. clxix. (1878) pp. 259, 260.

† Crookes, *Comptes Rendus*, lxxxiii. p. 1289; Proc. Royal Soc. xxv p. 312; 'Nature,' xv. p. 226 (1877).

layers is redly translucent. According to Tyndall*, a plate of rock-salt coated with lampblack, through which not a glimmer of the light from the most brilliant gas-flame could penetrate, transmitted 38 per cent. of the rays emanating from a vessel of boiling water.

Besides the absorption, the internal heat-conductivity of the substances made use of for radiometer-vanes is of exceeding importance, since it tends to remove the difference of temperatures of the two sides, and consequently to weaken the most important cause of the motion. Direct methodical experiments on this subject have not yet, so far as I know, been made, although differences in the action of metal and mica radiometers have frequently been referred to the difference of conductivity of the two substances. To investigate this more closely, the simple way presented itself of making, in my above-described apparatus, a series of similar experiments with mica and a metal. First the same mica plates which had served for the experiments on the influence of the glass case were smoked on one side over a turpentine-oil flame, and then introduced in the old way into the apparatus, after which the latter was completely exhausted. On irradiating the clear side with sunlight, it was so forcibly propelled against the light that the spot of light which served as index fell quite outside of the scale, and reading-off was impossible. At all events the lampblack, heated by absorption of the rays transmitted through the mica, acted just as if it had undergone direct heating.

On irradiation by the flame of a Bunsen burner, however, a peculiar phenomenon presented itself. Namely, at the first instant of the irradiation an energetic negative deflection occurred, as the irradiated clear side moved towards the source of light; then a brief oscillation to and fro followed; and after that the mica plate gradually passed over to a fixed position of rest corresponding to a positive deflection, consequently to a receding of the irradiated side of the mica. If the irradiation was then interrupted suddenly by the interposition of an opaque screen, the same play repeated itself in the opposite direction: first the vane was suddenly and energetically deflected with its blackened side going before; it then swung backwards and forwards for a few seconds; and after that it quite gently returned to its initial position of rest.

The following are the corresponding positions of the spot of light:—

* Tyndall, 'Heat as a Mode of Motion,' German translation, 3rd ed p. 407

With bright Gas-flame.							With dull Gas-flame.				
	L.	A.	L.	A.	L.	A.		L.	A.	L.	A.
I.	500	...	500	...	500		I.	500	...	500	
II.	585	85	587	87	642	142	II.	549	49	549	49
III.	462	38	462	38	485	15	III.	441	59	445	55
IV.	376	86	376	86	344	141	IV.	390	51	397	48
V.	500	...	500	...	500		V.	500	...	500	

Here the numbers in the columns superscribed with L. denote, in the horizontal rows:—

I. The resting-position of the index when the apparatus is shaded with a screen.

II. The extreme deflection of the index at the moment of the removal of the screen.

III. The resting-position of the index on full irradiation.

IV. The extreme deflection at the moment of shading.

V. The resting-position during the shading.

The numbers in the columns A. give the deflections from the next preceding resting-position, which correspond to the numbers standing on the left side of them.

As these deflections are not very great, and are pretty much of the same order as the previous ones produced by the absorption of the glass, it suggested itself that this absorption might exert a disturbing influence which would be likely to mask the proper motion produced by the vanes. On this account, in a second series of experiments the light, before arriving at the apparatus, was cleared of its most absorbable rays by three thick glass plates. The following results were then obtained (the notation having the same meaning as before):—

Bright Gas-flame.							Dull Gas-flame.		
	L.	A.	L.	A.	L.	A.		L.	A.
I.	500	...	500	...	500		I.	500	.
II.	587	87	585	85	584	84	II.	519	19
III.	477	23	482	18	483	17	III.	479	21
IV.	386	91	394	88	398	85	IV.	459	20
V.	500	...	500	...	500		V.	500	

It therefore appeared, as might have been foreseen, that by the filtration of the light in passing through the glass plates the first deflection, opposed to the action of the glass wall of the apparatus, in comparison with the final deflection was strengthened.

Further, in all the observations the almost absolute accordance in the magnitude of the first deflection, at the sudden commencement and sudden cessation of the irradiation, is remarkable.

With respect to the explanation of these phenomena, there cannot be any doubt that here two different and opposite causes of motion confront each other, of which the first comes in suddenly, the second gradually.

The first, suddenly acting cause is the absorption in the lampblack; the second, slowly appearing, is the heating of the front (mica) face by conduction. Here, truly, the peculiar case has happened, that that very substance which, as a bad conductor of heat, we had selected in order as far as possible to exclude the influence of conduction, shows this influence with special distinctness.

At the same time, however, this arrangement of the experiment permits us to separate completely the action of the absorption from that of the heat-conduction, because, in consequence of the comparatively slight conductivity of mica, a considerable time elapses before the effect of the conduction commences. The process is doubtless as follows:—The rays from the source of heat pass for the most part unabsorbed through the thin plate of mica, and are strongly absorbed in the coat of lampblack, in which, thin as it is, two sides are to be distinguished—the front side, nearest to the source, and the back side, furthest from it. The former absorbs more strongly, and becomes in consequence more heated, than the latter. Now, since the heat-conducting power of lampblack, as Rumford* already showed by experiment and H. Weber† concluded from his experiments on iron and German silver, is extraordinarily little (according to Rumford, almost exactly equal to that of sheep's wool), the heat of the front layer of the lampblack is conducted away less by the lampblack than by the mica; so that after a time the front face of the mica becomes warmer than the back of the lampblack. While this back side of the lampblack is at first suddenly heated by absorption and therefore produces the sudden first deflection of the vane, the heat conveyed to the mica by conduction causes

* Phil. Trans. 1792, p. 42. See also Tyndall, 'Heat' &c., German tr., 3rd ed. p. 276.

† Pogg. *Ann.* cxlvi. p. 282 (1872).

a gradual rise of temperature, and, in consequence of it, only a gradual deflection of the vane. If now, after the apparatus has, under the influence of the irradiation, taken a fixed position of rest (in which therefore the rotation-force generated by the excess of temperature of the front over the back side is kept in equilibrium by the torsion of the glass thread), the irradiation be suddenly interrupted, the back side of the lampblack suddenly loses the heat conveyed to it by absorption every instant during the irradiation, while the heat communicated to the mica face by conduction diminishes just as slowly as it increased at the beginning. Hence at the first instant of the shading the temperature-difference of the two faces is increased by the same quantity, although in the inverse direction, as at the first moment of the irradiation; and the increase produces an equally strong and sudden deflection of the vane as in the first case, only in the opposite direction. When then from the sudden cooling of the front layer of the lampblack a gradual lowering of temperature takes place at the front face of the mica, the difference between the two sides gradually comes to an end, and the vane slowly returns to its initial resting-position.

That this pregnant effect of heat-conduction is not brought to light with the usual mica radiometers is moreover a clear proof of their unsuitableness when the object is to fathom the causes of their motion. If, namely, such a radiometer is irradiated on one side so that the clear faces of the vanes is always illuminated and the dark sides are in the shade, the first deflection, produced by the absorption of the back side of the lampblack, is in every case sufficient to carry the irradiated vanes completely out of the range of the radiation and bring a fresh vane into it, which in its turn, by its first deflection, continues the rotation of the radiometer-cross in the same direction, so that a constant rotation, with the mica going in front of the lampblack, sets in, and the effect of the heat-conduction completely vanishes.

Therefore, although an effect of the heat-conduction had unequivocally come out already in the experiments with lampblackened mica vanes, yet corresponding experiments were undertaken with a very well conducting substance, in order to see whether the phenomena would be essentially affected by the good conductivity of the vane. For this purpose a vane of very thin copper foil (so-called Dutch metal), an excellent conductor of heat, was prepared and introduced into the bulb. First some experiments corresponding with the previously communicated observations with the translucent mica vane, were instituted with the vane not lampblackened, but bright on both sides.

On irradiation with sunlight, a positive deflection, with the irradiated side receding, occurred (the reverse of what took place with the mica). This phenomenon distinctly shows that, in spite of the good conducting-power and the thinness of the vane, absorption produced a lasting difference of temperatures between the front and the back side. That any difference which may have existed in the shape of the two sides was not the cause of the deflection was evident from the fact that irradiation of the other side of the vane had for its result a deflection in the contrary direction. The magnitude of this deflection amounted to about 50 divisions of the scale. A one-sided lateral irradiation of the glass case by sunlight produced, as might have been anticipated, no deflection, just as when the thin mica plate was employed.

When a bright gas-flame was employed as the source of heat a somewhat weaker deflection appeared than with sunlight, and which also had a quite different character.

While, namely, the sun produced a sudden deflection, here the deflection took place quite slowly, and thereby showed that its cause was a quite different one. In fact it appears to me very probable that in the copper foil only a very slight difference of temperature arises from the absorption of the rays emanating from the gas-flame, and that the greater part of the deflection is produced by the absorption in the glass case. An argument in favour of this is the fact that, with equal distance of the source of heat, the deflection was almost exactly the same on lateral irradiation of one half of the glass case as on full irradiation of the apparatus from the front. There resulted, namely, when the gas-flame was at 305 millim. distance from the vane, a deflection of the index,

On lateral irradiation, of . . . 23·5 millim.

„ irradiation from the front, of 23·0 „

The force produced by direct absorption in copper foil appears therefore to be almost exactly equal to that generated by the heating of the back part of the glass case.

Just as with the mica vane, here also by smoking the outside of the hinder half of the glass case the direction of the deflection was reversed—with sunlight directly, with the gas-flame by employing glass plates. Thereupon the Dutch-metal lamina was smoked on one side over an oil-of-turpentine flame and again put into the apparatus.

Irradiation of the bright side with sunlight gave a very strong positive deflection, in which the bright side receded until the smoked side began to come within the range of the radiation, whereupon a violent oscillation resulted.

If the bright side was irradiated by the flame of a Bunsen burner, a positive deflection again occurred, which varied between 100 and 200 scale-divisions, according to the distance and size of the flame.

On irradiating the smoked side, the Dutch metal behaved (as might have been foreseen) quite similarly to the mica; for both underwent a strong positive deflection.

The motion of the radiometer-vane can be produced not only by a difference in the substance and temperature of its two faces, but also by a difference in the shape, and, lastly, by a difference in the position with respect to the glass case. Many experiments have been made with curved vanes, especially by Crookes* and Zöllner†, which, however, do not completely agree with each other. Yet from all of these experiments it follows that the convex face corresponds to the blackened side in ordinary radiometers, the concave face to the bright side. Very peculiar were the results found by Crookes with aluminium vanes lampblackened, some on one and others on both sides; for whenever in those experiments any motion was generated, on employing a luminous source of heat the concave side went before, even when both sides were lampblackened but only the concave surface was irradiated. The perfect equality of temperature of both sides, assumed by Crookes in order to account for this phenomenon, appears to me, after the above-communicated experiments on the influence of the heat-conduction, very unlikely. Crookes, moreover, on Stokes's proposal, has made an experiment with a radiometer having peculiarly curved vanes, which shows it to be very probable that the rotation is not a result of the difference of form of the two sides by itself, but much rather arises from their different position to the glass case. The influence of the position of the vane with respect to the glass appears considerable in radiometers with transparent vanes placed obliquely; and in these it is found that the side which is nearest to the glass is repelled the most strongly. To this cause is certainly to be referred also the phenomenon remarked by Crookes‡, that a radiometer-cross rotates more rapidly in a small than in a large vessel.

THE LAWS OF RADIOMETER-MOTION.

The results of the experiments made by the various observers with radiometric apparatus cannot at once be comprised in general propositions from which it would be possible to

* Proc. Roy. Soc. xxv. pp. 312-314; *Comptes Rendus*, lxxxiii. pp. 1289-1291; *Nature*, xix. p. 88; *Phil. Trans.* 1878, pp. 294-302.

† Pogg. *Ann.* clx. pp. 160, 164, & 165.

‡ *Comptes Rendus*, lxxxiii. p. 1233; *Proc. Roy. Soc.* xxv. pp. 308, 309.

predict with certainty the course of each phenomenon. For, in the first place, in most of the experiments such different influences operated that even with exact knowledge of the laws that govern each one of them it would be difficult to calculate the result of their cooperation; and, besides, we are still far from being able to state numerically the properties in question of the different parts of the apparatus (such as absorptive capacity for the precise kind of radiation employed, emissive power, internal and external conductivity, specific heat, &c.); and much less still do we know of the laws by which the action in the apparatus itself of those various properties is regulated.

Nevertheless, in order to embrace in the simplest possible propositions the laws of radiometer-motion, we will first represent to ourselves the experimental conditions on which the occurrence of the motion is dependent. The first condition, which makes the apparatus (radiometer, otheoscope, or torsion-apparatus) what we call "radiometric," is that the gas enclosed in the instrument be rarefied beyond the neutral point. Then we get the following general proposition:—

Forces act upon the movable parts of a radiometric apparatus as soon as the equilibrium of temperature within it is disturbed.

A more particular statement, how and in what direction these forces act, upon what their magnitude depends, &c., cannot at present be made; but the experiments must be empirically combined into larger classes; and thus the following rules can be given:—

(1) A plane radiometer-vane tends, when irradiated, to recede with its warmer side.

(2) A curved vane tends, when warmed, to recede with its convex side.

(3) A vane suspended over against a warmed surface tends to recede from it.

By these propositions, however, little is gained, unless we can bring them under a common point of view that will make the process intelligible to us and show us on what it really depends.

We shall arrive at such a point of view if we consider what process in radiometric phenomena is precisely that with which the force comes in. The motion arises through the irradiation of the apparatus; the force of the motion must therefore be derived from the force of the radiation.

If now we investigate the successive changes produced in the apparatus by the irradiation, we very easily come to perceive that the motion of the vanes can only be brought about by heat passing from the vane to the gas, or *vice versa*; and,

in fact, all the theories of the radiometer which rest on other foundations have very soon proved themselves inapplicable.

If we consider radiometer-motion from this point of view, the first of the above propositions takes the form:—

An irradiated plane radiometer-vane tends to recede with that side which gives out the greatest quantity of heat to the gas.

Since it is difficult, in such propositions, to take into account the form of the cooperating bodies, we will rather fall back upon the surface-elements, and say:—

A surface-element which transfers heat to rarefied air, tends to recede with a force increasing with the intensity of the heat-current.

We are, indeed, in a position to state the law according to which the quantity of the heat given out increases. For Crookes* has shown, with the aid of a very sensitive torsion-apparatus in which a lampblackened pith bar served as the irradiated object, that when the apparatus was screened as much as possible from radiation outwards, the deflections increased in the inverse ratio of the square of the distance of the source of light from the pith. Now, as the force necessary for the torsion of the thread is proportional to the angle of torsion, the force produced by the radiation increases likewise in that proportion. The intensity of the radiation incident upon each surface-element of the pith bar, however, increases in precisely the same ratio, if the dimensions of the latter may be neglected in comparison with its distance from the source of light. Hence the quantity of heat absorbed by each element of the lampblack surface, and consequently the rise of temperature of each element, increases in the same measure. Now the quantity of heat given up to the surrounding air in an element of time by an element of surface is proportional to the difference of temperatures between the lampblack and the air; therefore the increase of the heat-current is proportional to the increase of temperature of the lampblack—that is, inversely proportional to the square of the distance of the source of light. But, as we have shown above, the force acting upon the entire pith bar, and consequently that upon each element of its surface, increases in the same proportion. Now, as this force vanishes when the quantity of heat transferred is *nil*, it is proportional to the quantity of heat given out by the element in unit time. We can therefore enunciate the proposition:—

An element of surface which is transferring heat to the rarefied air tends to recede with a force that is proportional to the intensity of the heat-current.

* Phil. Trans. clxvi. pt. 2.

Here the question immediately presses, whether a similar force will not arise at an element of the surface which, instead of giving heat to the gas, takes heat from it. This question is answered by proposition 3 (p. 119), if it be considered from the point of view of heat-conduction which we have gained for radiometer-motion universally. For the heat which is given out to the gas by the first warmed surface simply escapes from the gas again at the surface of the vane; and as it passes to the vane it imparts to it a receding motion. After the analogy of the above procedure, we can here also conclude that the repellent force is proportional to the quantity of heat passing. This agrees also with the experience that this force increases with the difference of temperatures between the heated surface and the gas and with the approach of the two surfaces to one another. We can therefore extend the above proposition and say:—

A surface-element at which heat enters or leaves the rarefied air undergoes a repulsion, of which the amount is proportional to the intensity of the heat-current.

In this proposition are contained not only the three rules above given, but all the propositions that can be enunciated on radiometer-motion. Thus, from it is directly inferred, as a consequence, the influence which a favourable position of the vane with respect to the glass case has upon the motion. For the nearer the vane confronts the glass, the more quickly do the air-particles give up their heat to the glass, and therefore the more quickly the conduction goes on, and hence the more heat is withdrawn from the vanes in the unit of time. Since Stokes, as already mentioned (p. 117), traced the phenomena occurring in radiometers with curved vanes to the favourable position of the convex side with respect to the glass case, our proposition contains also rule 2 (p. 120).

THEORY OF RADIOMETER-MOTION.

Till now we have sought to fathom the laws of radiometer-motion from the point of view that that motion is generated by the passage of heat between a surface and a gas, without forming any theoretical notion as to how such a passage of heat is capable of producing the motion. Now there are two quite different ways of explaining this—namely, the theory of air-currents, and those theories of the radiometer which rest upon the kinetic theory of gases.

The view that the motions are due to air-currents* can hardly be justified; for as the analogous phenomena under

* Neesen, Pogg. Ann. clvi. pp. 144–156 (1875), clx. pp. 143–153 (1877). O. E. Meyer, *Kinetische Theorie der Gase*, Breslau, p. 154 (1877).

atmospheric pressure take place in exactly the contrary way to the motions of the radiometer, in order to maintain these theories it would be necessary to assume that in rarefied air the currents follow quite different laws from those governing them under the ordinary pressure. At the most one might infer these laws from the radiometer-motions, and so would fall into a vicious circle. Moreover it is indubitable that air-currents arise in radiometers; but these, in the high rarefactions, are very weak, and they are not the cause of the motion, but usually counteract it.

Of the impossibility of finding the cause of the motion in air-currents one can easily be convinced by a very simple phenomenon which I have had occasion to observe. A radiometer (or, more correctly, an otheoscope) possessed a movable cross with four mica vanes inserted radially and inclined about 45° to the plane of the horizon, which were lampblackened on their upper side. Immediately over this cross was a circular disk of mica, easily rotated about a vertical axis, suspended horizontal. Horizontally incident sunlight produced rotation of the cross with the bright side preceding, and an opposite rotation of the disk.

This same motion continued when a portion of the cone of light was cut off so that only the lampblackened sides of the vanes were irradiated, while the bright sides were in the shade. When, on the contrary, the lampblackened sides of the vanes were shaded and the bright sides irradiated, the motion of the cross was reversed, so that now the blackened sides preceded. But the mica disk preserved its previous direction of rotation; and consequently now the vanes and the disk rotated in the same direction. Now it is certainly inconceivable that, in consequence of the change of the illumination, the air-current at one place should be reversed while that immediately above it has the same direction as before.

From a much more sure foundation, and one that has already by manifold experiments been rendered almost a certainty, do those theories start which rest on the kinetic theory of gases. The thought which is common to all these theories is that the "vacuum" with which we have to do in radiometers is not an empty space at all, but still contains an enormous number of molecules of air. If, then, these molecules meet a surface of higher temperature, a part of the heat contained in the surface passes to the gas-molecules; these become warmer; that is, according to the kinetic theory, they rebound with greater velocity than they approached. According to the law of the equality of action and reaction, the warmer surface must suffer a greater repulsion from these particles retiring with accele-

rated velocity than if it possessed the same temperature as the surrounding gas. In consequence of this the warmer side of the radiometer-vane is exposed to a greater repulsion or pressure than the cooler side, and therefore moves backwards. Just so these warmed gas-molecules repel a surface on which they impinge, as they give up to it their surplus velocity.

Moreover, this explanation requires our proposition on radiometer-motion given above (p. 121) to be somewhat modified, since according to it a surface-element always undergoes repulsion, even when it has the same temperature as the gas. But as in reality a surface is never exposed to motion by the gas without its back side being likewise in the rarefied gas, the modification is in practice superfluous.

We have not yet explained how it is that the motion does not commence until the gas has reached a certain degree of rarefaction; and this is the point in the explanation of which the different theories essentially differ. Of all these theories, that framed by Osborne Reynolds, communicated and adopted by Schuster*, seems to me to come nearest the truth. It rests essentially on the assumption that the predominant motion in a determined direction communicated to the gas-molecules by the passage of heat from a warmer body to the gas cannot be again withdrawn from the gas by the collisions of its molecules, but is only withdrawn when the motion passes again from the gas to a solid body. Now, at the places where the unilateral motion enters and leaves the gas certain forces become operative.

This theory agrees perfectly with the above-given general law of radiometer-motion; and the motion which appears in the solid body is quite simply accounted for by the impact of the gas-particles and the reaction of the case. That at the same time the force acting upon an element of the surface is proportional to the intensity of the heat-current follows immediately from our assumption if it be presupposed that the rarefaction is so great that, on both sides of the vane, each surface-element is struck by an equal number of molecules with the same mean velocity. Let the number be n , and the component perpendicular to the surface-element, of the mean velocity of the impinging molecules, v ; then, assuming that one side of the vane has the same temperature as the gas, and putting the mass of a molecule = 1, the quantity of motion communicated to the surface-element on this side in unit time is equal to $2nv$. If on the warmer side of the vane the molecules rebound with the mean normal velocity-component

* Nature, xvii. p. 143 (1877).

$v + \Delta v$, the quantity of motion here communicated to the surface-element in the unit of time is $2n(v + \Delta v)$; therefore the force which becomes effective is $2n\Delta v$; that is, proportional to the heat-motion given up to the gas by the surface-element.

As to the reversed motion at higher pressure, this is, no doubt, produced by the air-currents arising on the warmer side, which become weaker and weaker as the rarefaction increases, until at last, at the neutral point, they exactly counterbalance the contrary force of the air-molecules. Since these air-currents differ in strength and direction according to the form of the surface at which they arise, the conductivity of the warm substance, the shape of the vessel, &c., the dependence of the position of the neutral point on all these is thus accounted for.

The complicated phenomena in the so-called electrical radiometers cannot, so far as they take place in the highest rarefactions, well be regarded as analogous heat-effects upon the air-molecules, but are much rather conditioned by the laws of electrical phenomena in highly rarefied-air spaces, so that their investigation, though of importance for the explanation of those laws, is not so for that of radiometer-motion.

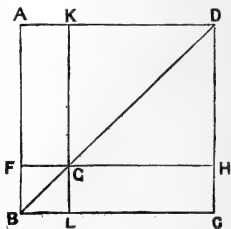
XVIII. On the Graphic Representation of the Law of Efficiency of an Electric Motor. By PROFESSOR SILVANUS P. THOMPSON*.

(1) **V**ARIOUS graphic constructions have been given at different times to represent the work performed by an electric motor and the electric energy expended upon it. The main defect of those hitherto given has been that they present these quantities in such a manner that a comparison of the two, which would show the efficiency of working of the motor, is not immediately evident. Moreover it has not been possible hitherto to show on one construction both the law of maximum rate of working and the law of efficiency. The following construction makes them evident to the eye.

Fig. 1.

Let the vertical line AB (fig. 1) represent the electromotive force, E , of the electric supply when the motor is at rest. On AB construct a square $ABCD$, of which the diagonal BD may be drawn. Now measure out from the point B , along the line BA , the counter electromotive force of the motor e ; this quantity will increase as the velocity of the motor increases.

Let e attain the value BF . Let us inquire what the actual



* Communicated by the Physical Society.

current will be, and what the energy of it ; also what the work done by the motor is.

First complete the construction as follows :—Through F draw F G H parallel to B C, and through G draw K G L parallel to A B. Then the actual electromotive force at work in the machine producing a current is $E - e$, which may be represented by any of the lines A F, K G, G H, or L C. Now the electric energy expended per second is EC ; and

$$\text{since } C = \frac{E - e}{\Sigma R}, \quad \frac{E(E - e)}{\Sigma R};$$

and the work absorbed by the motor, *measured electrically*, is

$$\frac{e(E - e)}{\Sigma R}.$$

ΣR being a constant, the values of the two may be written respectively

$$E(E - e)$$

and

$$e(E - e).$$

Now the area of the rectangle

$$AFHD = E(E - e),$$

and that of the rectangle

$$GLCH = e(E - e).$$

The ratio of these two areas on the diagram is the efficiency of a perfect motor, under the condition of a given constant electromotive force in the electric supply.

(2) So far we have assumed that the efficiency of a motor (working with a given constant external electromotive force) is to be measured electrically. But no motor actually converts into useful mechanical effect the whole of the electric energy which it absorbs, since part of the energy is wasted in friction and part in wasteful electromagnetic reactions between the stationary and moving parts of the motor. If, however, we consider the motor to be a *perfect* engine (devoid of friction, not producing wasteful Foucault currents, running without sound, giving no sparks at the collecting-brushes, &c.), and capable of turning into mechanical effect 100 per cent. of the electric energy which it absorbs, then, and then only, may we take the electrical measure of the work of the motor as being a true measure of its performance. Such a “perfect” electric engine would, like the ideal “perfect” heat-engine of Carnot, be perfectly reversible. In Carnot’s heat-engine it is

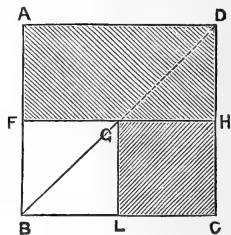
supposed that the whole of the heat actually absorbed in the cycle of operations is converted into useful work; and in this case the efficiency is the ratio of the heat absorbed to the total heat expended. As is well known, this efficiency of the perfect heat-engine can be expressed as a function of two absolute temperatures, namely those respectively of the heater and of the refrigerator of the engine. Carnot's engine is also ideally reversible; that is to say, capable of reconvertng mechanical work into heat.

The mathematical law of efficiency of a perfect electric engine illustrated in the above construction is an equally ideal case. And the efficiency can also be expressed, when the constants of the case are given, as a function of two electromotive forces. We shall return to this comparison a little later.

The Law of Maximum Rate of Working (Jacobi).

(3) Let us next consider the area $GLCH$ of the diagram (fig. 2), which represents the work utilized in the motor. The value of this area will vary with the position of the point G , and will be a maximum when G is midway between B and D ; for of all rectangles that can be inscribed in the triangle BCD , the square will have maximum area (fig. 2). But if G is midway between B and D , the rectangle $GLCH$ will be exactly half the area of the rectangle $AFHD$; or, the useful work is equal to half the energy expended. When this is the case, the counter electromotive force reduces the current to half the strength it would have if the motor were at rest; which is Jacobi's law of the efficiency of a motor doing work at its greatest possible rate.

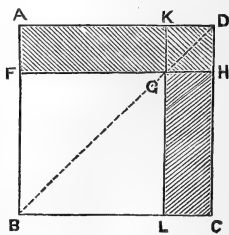
Fig. 2.



Law of Maximum Efficiency.

(4) Again, consider these two rectangles when the point G moves indefinitely near to D (fig. 3). We know from common geometry that the rectangle $GLCH$ is equal to the rectangle $AFGK$. The area (square) $KGH D$, which is the excess of $AFHD$ over $AFGK$, represents therefore the electric energy which is wasted in heating the resistances of the motor. That the efficiency should be a maximum the heat-waste must be a minimum. The

Fig. 3.



ratio of the areas $A F H D$ and $G L C H$, which represents the efficiency, can therefore only become equal to unity when the square $K G H D$ becomes indefinitely small—that is, when the motor runs so fast that its counter electromotive force e differs from E by an indefinitely small quantity only.

Further, it is clear that if our diagram is to be drawn to represent any given efficiency (for example, an efficiency of 90 per cent.), then the point G must be taken so that area $G L C H = \frac{9}{10}$ area $A F H D$; or, G must be $\frac{9}{10}$ of the whole distance along from B towards D . This involves that e shall be equal to $\frac{9}{10}$ of E ; which expresses geometrically the law of maximum efficiency.

It is strange that even in many of the accepted text-books this law is ignored or misunderstood. It is indeed frequent to find Jacobi's law of maximum rate of working stated as the law of efficiency. Yet as a mathematical expression the law has been known for many years. It is implicitly contained in more than one of the memoirs of Joule; it is implied also in more than one passage of the memoirs of Jacobi*; it exists

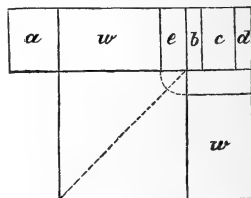
* Jacobi seems very clearly to have understood that his law was a law of maximum working, but not to have understood that it was not a law of true economical efficiency. In one passage (*Annales de Chimie et de Physique*, t. xxxiv. (1852) p. 480) he says:—"Le travail mécanique maximum, ou plutôt l'effet économique, n'est nullement compliqué avec ce que M. Müller appelle les circonstances spécifiques des moteurs électromagnétiques." Yet, though here there is apparently a confusion between the two very different laws, in a preceding part of the very same memoir Jacobi says (p. 466):—"En divisant la quantité de travail par la dépense (de zinc), on obtient une expression très-importante dans la mécanique industrielle: c'est l'effet économique, où ce que les Anglais appellent *duty*." Here, again, is a singular confusion. The definition is perfect; but "effet économique" is not the same thing as the maximum power. Jacobi's law is not a law of maximum efficiency, but a law of maximum power; and that is where the error creeps in. It is significant, in suggesting the cause of this remarkable conflict of ideas, that throughout this memoir Jacobi speaks of *work* as being the product of force and velocity, not of force and displacement. The same mistake—common enough amongst continental writers—is to be found in the accounts of Jacobi's law given in Verdet's *Théorie mécanique de la Chaleur*, in Müller's *Lehrbuch der Physik*, and even in Wiedemann's *Galvanismus*. Now the product of force and velocity is not work, but work divided by time—that is to say, rate-of-working, or "power." This may account for the widely-spread fallacy. Jacobi makes another curious slip in the memoir above alluded to (p. 463), by supposing that the strength of the current can only become $=0$ when the motor runs at an infinite speed. We all know now that the current will be reduced to zero when the counter electromotive force of the motor equals that of the external supply; and if this is finite, the velocity of the motor, if there is independent magnetism in its magnets, need also only be finite. This error—also to be found in Verdet—seems to have thrown the latter off the track of the true law of efficiency, and to have made him fall back on Jacobi's law.

in the *Théorie Mécanique de la Chaleur* of Verdet*. Yet it remained a mere mathematical abstraction until its significance was pointed out three or four years ago by Siemens.

(5) Further, if the motor be not a "perfect" one, but one whose intrinsic efficiency, or *efficiency per se*, is known, the actual mechanical work performed by the motor can be represented on the diagram by simply retrenching from the rectangle $GLCH$ the fraction of work lost in friction &c. Similarly, in the case where the electric energy expended has been generated in a dynamo-electric machine whose intrinsic efficiency is known, the total mechanical work expended can be represented by adding on to the area $A F H D$ the proportion spent on useless friction &c. To make the diagram still more expressive, we may divide the area $K G H D$ into slices proportional to the several resistances of the circuit; and the areas of these several slices will represent the heat wasted in the respective parts of the circuit. These points are exemplified in fig. 4, which represents the transmission of power between two dynamos, each supposed to have an intrinsic efficiency of 80 per cent., each having 500 ohms resistance, working through a line of 1000 ohms resistance, the electromotive force of the machine used as generator being 2400 volts, and the counter electromotive force of the machine used as motor being 1600 volts.

The entire upper area represents the total mechanical work expended. Call this 100, and it is expended as follows:— $a=20$, lost by friction &c. in the generator; $b=6\frac{2}{3}$, lost in heating generator; $c=13\frac{1}{3}$, lost in heating line-wires; $d=6\frac{2}{3}$, lost in heating motor; $e=10\frac{2}{3}$, lost in friction in the motor; $w=42\frac{2}{3}$ is the percentage realized as useful mechanical work.

Fig. 4.



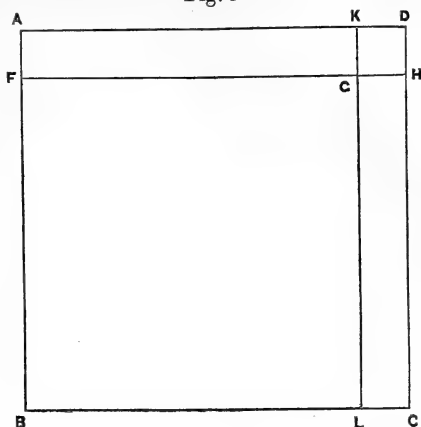
(6) The advantage derived in the case of the electric transmission of power from the employment of very high electromotive forces in the two machines is also deducible from the diagram.

Let fig. 3, given above, be taken as representing the case where E is 100 volts and e 80 volts. Now suppose the resistances of the circuit to remain the same while E is increased to 200 volts and e to 180 volts. (This can be accomplished by increasing the speed of both machines to the requisite degrees.) $E-e$ is still 20 volts, and the current will be the same as before. Fig. 5 represents this state of things. The square

* Verdet, *Œuvres*, t. ix. p. 174.

K G H D which represents the heat-waste is the same size as before; but the energy spent is twice as great, and the useful work done is more than twice as great as previously. High electromotive force therefore means not only a greater quantity of power transmitted; but a higher efficiency of transmission also. The efficiency of the system in the case of fig. 3

Fig. 5.

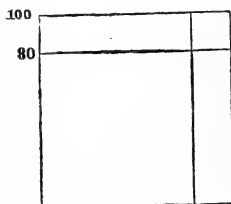


was 80 per cent; in the case of fig. 5 it is 90 (the dynamos used being supposed "perfect"); and whilst double energy is expended, the useful return has risen in the ratio of 9 to 4.

(7) So far it has been supposed that the resistance of the system is a constant quantity. But it is possible to construct diagrams in which changes of resistance are taken into account. All that is necessary is to vary the scale of the diagram, the linear unit of scale being chosen inversely proportional to the square root of the total resistance. This will make the areas of the diagrams inversely proportional to the resistances in the different cases, as required by the law that the energy of the current is proportional to $\frac{E^2}{R}$.

An example in which this rule is applied is the following. It can be shown that the power transmitted and efficiency of a transmitting system are increased by doubling the number of coils in the armatures of the machines. This is not at first sight self-evident; for though, *ceteris paribus*, this doubles the electromotive force of the machines, it also doubles their resistances. Let fig. 6 be the diagram for a transmitting system, where $e = \frac{1}{2}E$, and in which these values are both going to be doubled by doubling the number of armature-coils. There are two cases to consider:—(a) first, where the line-resistance is very small compared with that of the two machines; (b) second, where the line-resistance is very large compared with that of the two machines.

Fig. 6.



(a) In the former case, where we neglect the resistance of the line, we must draw a diagram diminishing the linear unit of scale to $\frac{1}{\sqrt{2}}$ of its value. But as on this scale we are going

to represent doubled electromotive forces, the actual figure will have to be $\sqrt{2}$ times as large as fig. 6. Draw, then, the

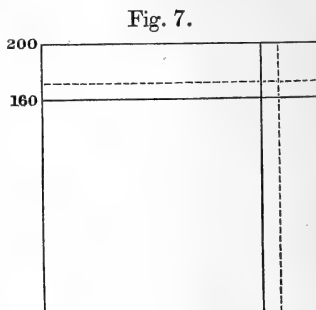
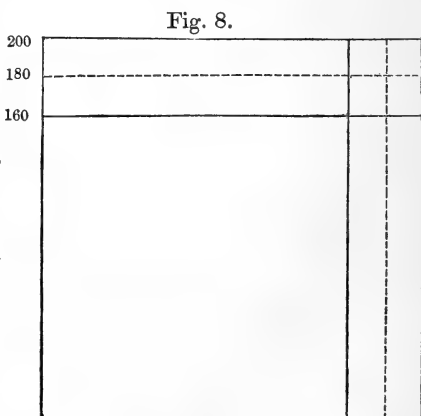


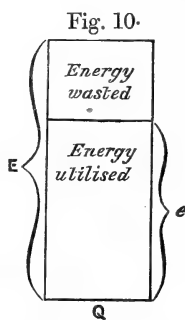
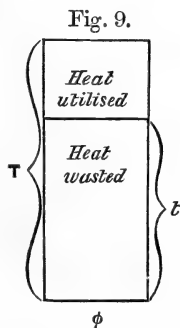
fig. 7, taking as side of the square a line equal to the diagonal of fig. 6, and we obtain a diagram in which, while the efficiency is the same as before, the actual quantity of work effected in unit time is doubled. For the areas representing respectively energy expended, work done, and heat-waste are in fig. 7 double of those in fig. 6. But no such case can occur in practice, as the line must have some resistance. Then doubling the number of coils of the machines will not cut down the scale so greatly as we have supposed; and the work transmitted will be more than doubled. Further, if the number of coils on the machine used as motor be a little more than doubled, a higher efficiency will be attained; since then the area of the square K G H D will be further diminished, while the scale on which the diagram is drawn will only be very slightly diminished. If diminished, as shown by the dotted lines, so that $e - E$ has the same value as before, the efficiency will be a little less than doubled, the power transmitted remaining as at first.

(b) If the case where the line-resistance is very great as compared with the resistances of the machines be taken, we find that doubling the number of the coils of the two machines will double their respective electromotive forces, without altering appreciably the total resistance or the scale of the diagram. To represent this change relatively to fig. 6, we must reconstruct that figure, doubling its linear dimensions each way, as in fig. 8.



It is at once evident that the power transmitted is increased fourfold, while the efficiency remains the same. If we increase the number of coils, as before, on the machine at the receiving end of the line so as to bring up the difference $E - e$ to the value it had in fig. 6, the scale of the diagram will still be unaltered; the power transmitted will be now only double instead of quadruple; but the efficiency will thereby be more than doubled, the heat-waste being the same, and the energy utilized more than twice as great. High electromotive force is therefore advantageous in both cases, especially in the case of a great resistance in the line.

(8) It only remains to point out a curious contrast that presents itself between the efficiency of a perfect heat-engine and that of a perfect electric engine. We saw (§ 2) that the one could be expressed as a function of two temperatures, whilst the other could be expressed as a function of two electromotive forces. But in the heat-engine the efficiency is the greatest when the difference between the two temperatures is a maximum; whilst in the electric engine the efficiency is the greatest when the difference between the two electromotive forces is a minimum. The two cases are contrasted in figs. 9 and 10, fig. 9 showing



the efficiency of a heat-engine working between temperatures T and t (reckoned from absolute zero); whilst fig. 10 shows the efficiency of an electric engine receiving current at an electromotive force E , its counter electromotive force being e . Joule's remark, here illustrated, that an electric engine may be readily made to be a far more efficient engine than any steam-engine, is amply justified by all experience. But in spite of this fact, electric engines are, as yet, dearer in practice than heat-engines, simply because energy in the form of electric currents supplied at a high potential is, as yet, much more costly to produce than energy in the form of heat supplied at a high temperature.

XIX. *On the Change in the Double Refraction of Quartz produced by Electric Forces.* By Prof. W. C. RÖNTGEN*.

IT is well known that Sir W. Thomson has endeavoured to explain pyroelectric phenomena by assuming that the interior of pyroelectric crystals is constantly in a condition of electric polarization; the external action of this polarization is neutralized by a constant electric charge of the surface, so long as the polarization remains unaltered. Changes in the temperature of the crystals alter this condition; and the pyroelectric phenomena observed are the consequence of this change.

This view is supported by the phenomena recently observed by Messrs. J. and P. Curie†, confirmed by Hankel‡, and distinguished as *piezoelectric*, as well as by the experiments made by J. and P. Curie§ on the changes of form produced by electricity in pyroelectric crystals; at least these phenomena are naturally explained by the hypothesis in question.

I will not now dwell upon the difficulties which, in my opinion, militate against the acceptance of this hypothesis, but will simply explain how this view induced me to make the experiments here described, the results of which are certainly in themselves worthy of attention. The consideration from which I started was the following:—If an electric polarization were constantly present in a pyroelectric crystal in definite directions, and if it is allowable to conclude from the recently discovered effects of statical electricity on the optical properties of singly-refracting media that not only the polarization produced by external electric forces, but also any natural polarization already existing, would exert an influence on the vibrations of light transmitted through the crystal, then the optical properties of a pyroelectric crystal would be affected in different ways, according as the natural polarization was weakened or strengthened by the action of external electric forces.

Taking a quartz crystal as example, the result of piezoelectric experiments with it may be described as follows, at least for normal crystals of simple formation:—A section of the crystal at right angles to the principal axis may be divided by three straight lines intersecting each other in any point

* Translated from a separate impression from the *Berichte der Oberh. Ges. für. Natur- und Heilkünde*, communicated by the Author.

† *Compt. Rend.* xci. pp. 294, 383 (1880); xcii. pp. 186, 350; xciii. p. 204 (1881).

‡ *Abhandl. der kön. sächs. Gesellschaft.* vol. xii. p. 459 (1881).

§ *Compt. Rend.* xciii. p. 1137 (1881).

at angles of 60° , into six fields, which possess the following properties :—A pressure exerted upon the crystal in any direction through the point, or in any parallel direction, causes the crystal to become electrified at the two points of pressure, the one becoming positive, the other negative. If we change from one direction of pressure to another lying in an adjacent field, the sign of the electricity at the points of pressure changes whenever the direction of pressure crosses the boundary between adjacent fields.

Hence it follows that a pressure exerted in the direction of one of the three lines mentioned can produce no piezoelectricity ; on this account I propose to call these three directions the axes of no piezoelectricity. In the three directions bisecting the angles between these axes, there must be a maximum of piezoelectricity produced ; these directions may therefore be called the axes of maximum piezoelectricity. They coincide more or less exactly with the so-called secondary axes, the lines joining the two opposite edges of the quartz crystal. I am not yet able to decide whether or not they coincide exactly, since the experiments which I have made to test the point are not sufficiently numerous ; with some crystals, however, it seems to be really the case. If this were so, the axes of no piezoelectricity would have the same direction as the intermediate axes of the quartz.

Let us suppose that the three axes of maximum piezoelectricity give the three directions of the natural polarization : if we consider the ends of each axis as positive or negative, corresponding to the natural distribution of electricity in the interior, then these ends, if we follow them round in order, must be alternately positive and negative. The electricity produced by pressure is correspondingly positive or negative ; and this holds good, as already remarked, for the whole field in which any axis lies.

If now a piece of quartz be so exposed to the inductive action of static electricity, that at any place the lines of force run at right angles to the principal axis, and at the same time do not run in the direction of an axis of no piezoelectricity, these forces will produce an increase or decrease of the natural polarization at this place, and with it, in accordance with the hypothesis stated at the outset, we shall have an increase or a decrease of the natural double refraction of rays which traverse the crystal at right angles to the principal axis and to the lines of force. The occurrence of the one case or of the other will depend wholly upon which of the three pairs of opposite fields the direction of the lines of force lies in, and in what direction they traverse it. There

would be no change of the natural double refraction to be observed under the conditions assumed, if the lines of force run in the direction of one of the three axes of no piezoelectricity.

These conclusions, that the double refraction of the quartz may be increased or diminished at pleasure by the action of statical electricity, and that under certain defined conditions the double refraction is incapable of any such change, have been found to be confirmed by experiment.

The preliminary experiments were made with two rectangular parallelepipeds of pure Brazilian quartz, in which optical experiment showed no deformation. The pieces of quartz, which were obtained from Messrs. Steeg and Reuter, were 2.0 centim. long, and were made exactly of a uniform width and breadth of 1.2 centim. According to my instructions, the longest axis of the parallelepipeds should have coincided with a secondary axis; but, owing to a misunderstanding on the part of the workman, little weight was attached to this condition. Subsequent inquiries, as well as determinations made by means of Leydolt's* etched figures, showed that in both pieces this direction deviated but little from that of a secondary axis. It may further be remarked that it is sufficient for the investigations in hand that the direction of length should not coincide with an axis of no piezoelectricity: the piezoelectric experiments have shown that this was the case. Two of the lateral surfaces are exactly at right angles to the principal axis; and the other two lateral surfaces are therefore strictly parallel to the principal axis and nearly parallel to a secondary axis.

Each piece had a perforation in the direction of its length of about 0.2 centim. width, starting from the centre of the end faces; the coaxial perforations do not quite reach each other in the middle of the crystal, but leave a thickness of 0.2 centim., which forms the portion of the crystal whose electro-optical properties are to be investigated.

Both crystals were examined; but in each experiment only one crystal was placed in the electric field; the other was used to compensate the natural double refraction of the first: for this purpose the pieces were cemented together with a little isinglass, so as to have their principal axes at right angles to each other. Light polarized in a plane making an angle of 45° with the principal axes traversed the crystal at right

* *Berichte der Wiener Akademie*, vol. xv. p. 59 (1855).

angles to the plane containing the principal axis and a secondary axis of one of the crystals, and consequently at right angles to two lateral surfaces.

When placed between crossed Nicols, the centre of the field of view (that is, the place between the perforations) was nearly uniformly dark (employing sodium-light, the intensity of which was quite sufficient for these experiments), not regarding certain small irregularities, probably resulting from pressure during boring. Brass wires of about 0.15 centim. thickness, with well-rounded ends, were placed in the two perforations of the crystal under examination, which were so connected with the electrodes of a Holtz machine as to admit of rapid reversal of the connexions. The difference of potential between the two electrodes could be varied at pleasure and continuously, whilst the machine was rotated with uniform velocity by means of a secondary connexion containing a variable air-resistance. This method, which I have employed for some time, consists in connecting one electrode with an insulated sharp point, the other with an insulated metal plate. The point and plate are opposed to each other; and their distance apart can be altered at pleasure; the further they are apart the greater is the resistance of air for the dark discharge, and the greater therefore is the difference of potential.

In order to avoid the undesirable passage of sparks between the wires within the crystal, which is liable to occur when the difference of potential becomes too great, the crystal was placed in a small flask filled with sulphide of carbon, and in the later experiments with benzol; the polarized light entered one side at right angles where the flask was perforated, and the opening closed by a piece of plate-glass, and passed out at the opposite side, which was perforated in a similar manner.

For the purpose of control I have also examined the pieces of quartz in air, and have observed in all essential points the same behaviour as when they were immersed in sulphide of carbon or in benzol.

The direction of the secondary axis, the axis of length of the crystal examined for electro-optical effect, was placed vertical; the direction of the lines of force in the middle of the crystal was therefore vertical; and the principal plane of the Nicol consequently made angles of 45° with these lines of force (the arrangement previously designated "Position I." of the Nicol*).

* Compare *Ber. d. Ob. Ges.* vol. xix. p. 1 (1880); *Wied. Annal.* vol. x. p. 77 (1880).

To distinguish the pieces of quartz from each other they may be called "Crystal I." and "Crystal II."; one end face of each is marked, and in what follows is called "the marked end."

The action which the electricity exerts upon the light passing through the crystal was compared with the action which was caused by the compression, in a vertical or horizontal direction, of a piece of glass inserted between the analyzer and crystal. If we have noted, for example, as follows,

"Below +, above —; the same action as vertical compression,"

it is to be understood that charging the ends of the secondary axis below with positive and above with negative electricity produced the same optical change in the centre of the field of view as a compression of the plate of glass in a vertical direction.

Experiment I. Crystal I., marked end at the bottom.

Below +, above —; same action as *vertical* compression.

Below —, above +; ,, *horizontal* ,,

Experiment II. Crystal I., marked end at the top.

Below +, above —; same action as *horizontal* compression.

Below —, above +; ,, *vertical* ,,

Experiment III. Crystal I., marked end *above*.

(a) The centre of the field of view was made somewhat darker by vertical compression of the glass plate, then, while the glass plate remained compressed, "Below +, above —" caused an increase of brightness. This action could be compensated by stronger compression in a vertical direction.

(b) By vertical compression of the glass plate the centre of the field of view was again rendered darker; "Below —, above +" again rendered the field of view brighter; but this brightening was not now compensated, but rather increased by stronger compression in the vertical direction.

Experiment IV. (after the crystals had been cemented together in the opposite way). Crystal II., marked end at the top.

Below +, above —; same action as *horizontal* compression.

Below —, above +; ,, *vertical* ,,

Experiment V.—In the above experiments the quartz was surrounded by sulphide of carbon; in the following ones it was surrounded by air.

Crystal I., marked end at the *top*.

Below +, above -; same effect as *horizontal* compression.

Below -, above +; ,, *vertical* ,,

Experiment VI. Crystal II., marked end at the *top*.

Below +, above -; same effect as *horizontal* compression.

Below -, above +; ,, *vertical* ,,

The experiments were repeated at very different times and under varied conditions: thus in the later experiments there was often only one crystal in the flask filled with benzol, the other, which served as compensator, being surrounded by air; sometimes plates of mica or other pieces of quartz were chosen as compensator; but the same results described above were always obtained, and never any thing different.

It is well known that a compressed glass plate behaves optically as a negative crystal the principal axis of which coincides with the direction of compression. Since quartz is a positive crystal, the results obtained above may be expressed by saying that the double refraction of the pieces of quartz examined increased when the marked end of the secondary axis was charged with positive electricity and the unmarked end with negative, and that, on the contrary, the double refraction diminishes when the marked end of the axis is negative and the unmarked end positive.

It was now further examined how these ends behave in piezoelectric relationship. The experiment was made by covering the end faces of the crystal with tinfoil and compressing it in a screw-press between plates of ebonite in the direction of its length: the one strip of tinfoil was connected with a delicate Fechner's goldleaf electroscope, capable of indicating the charge of the insulated pole of a Daniell's element by a marked deflection, the other was connected with earth.

The experiment frequently repeated gave uniformly the same result, that the marked end of the secondary axis of both pieces of quartz became negatively electrified upon increase of pressure, and positively electrified upon decrease of pressure; the unmarked end became respectively positively or negatively electrified.

We may therefore express the result of the electro-optical investigation thus: The double refraction of quartz increases if positive electricity is communicated to that end of a secondary axis which, upon increase of pressure acting in the direction of this secondary axis, becomes negatively electrified, and if at the same time negative electricity is communicated

to the other end. The double refraction diminishes, on the other hand, when the distribution of the communicated electricities is the opposite.

If we adopt the view that a piezoelectric crystal is in a condition of electric polarization, whose direction, in the particular case of quartz, seems to coincide with the direction of a secondary axis, and that the observed piezoelectricity is a consequence of the change of polarization produced by pressure, then simple reasoning shows that the end which becomes negative upon increase of pressure is that towards which the negative side of the electrically polarized molecules is turned. But we found before that the double refraction increases when positive electricity is communicated to this end and negative electricity to the other; the polarization must be strengthened by this electrification; and we consequently obtain the result, that the double refraction of quartz increases or decreases according as the natural polarization is increased or weakened by external electric forces.

Having thus found the first of the consequences mentioned at the beginning confirmed by experiment, I proceeded further to put the second to the experimental test. The experiments described had shown me that, as far as intensity was concerned, there was at any rate no great difference between the increase and the decrease of the double refraction produced by equal electric forces; thence I concluded that it must be possible to find a direction in the quartz possessing the property that electric forces acting in this direction would produce no perceptible change in the double refraction. From what has been said above, this direction was to be sought for in an axis of no piezoelectricity—consequently in, or at any rate in the neighbourhood of, an intermediate axis of the crystal. I obtained therefore from Messrs. Steeg and Reuter a square plate of quartz of 1·5 centim. in the side and 0·25 centim. thickness, which was cut accurately parallel to a side face, and consequently at right angles to an intermediate axis. The principal axis is parallel to a side of the square; a secondary axis is consequently parallel to one of the sides at right angles to the first named. The four narrow side faces are polished.

First of all I examined whether the intermediate axis of this crystal was really an axis of no piezoelectricity. The result obtained was that even great changes of pressure in the direction of the intermediate axis gave rise to no perceptible quantities of electricity at the points of pressure, and that this direction is consequently an axis of no piezoelectricity. It may be remarked that a pressure parallel to the principal

axis gave the same result, that, on the contrary, a pressure parallel to the secondary axis produced considerable quantities of electricity.

Next the plate was perforated in the centre of a square end face so that a nearly hemispherical depression was formed there (depth 0.1 centim). A further examination for piezoelectricity gave the same result as before.

The plate so prepared was placed upon the brass plate employed in my former electro-optic experiments between two thin strips of glass cemented to it, and introduced into the little flask filled with benzol. The end of a well-rounded brass wire projected into the depression in the plate, and formed the upper electrode, the plate being the lower electrode. The rays of light traversed the crystal parallel to the secondary axis, and consequently at right angles to the principal axis and to a secondary axis. The Nicols were placed in position I.

In order to compensate the natural double refraction, I employed the same means as in the experiments with the quartz parallelepiped; a second square plate of quartz, which was cut at right angles to the principal axis and had nearly the same dimensions as the first, was placed between the analyzer and glass flask, upon a stand movable about three axes at right angles to each other, and so placed that its principal axis was at right angles to that of the first plate. The double refraction could not be exactly compensated; but it was very easy to decide whether any such change of double refraction as took place with the first plate occurred here, by observing one of the vertical dark bands which crossed the field of view when the compensating plate was turned a little about a vertical axis. Any change in the double refraction would have been perceived by a displacement of the band to the right or to the left.

The experiment was made by adjusting a band in the middle of the field of view, consequently exactly under the bulb-shaped depression, which was to see if any displacement occurred when the difference of potential between the electrodes was subjected to rapid change. I have never been able to observe any such displacement, however varied the conditions under which the experiment has been made. Hence it follows that a change in the double refraction is not produced in any perceptible degree by electric forces acting in the direction of the axis of no piezoelectricity*.

* It is obvious that I cannot assert that no trace of electro-optic effect could have been observed in the direction of an axis of no piezoelectricity if much greater differences of potential than those in my experiments,

The bands were next adjusted first to the left and then to the right of the depression, but remaining close to it; in these positions also no effect of electrification upon the double refraction was observed; neither the lower nor the upper end of the band altered its position.

The observation that also the upper end of the band was not displaced is of importance; for since the lines of force which issue from the hemispherical depression run there horizontally, and coincide with the direction of the principal axis to the left and to the right of the upper electrode, it follows that also in the direction of the principal axis of the quartz no perceptible change can be produced in the double refraction by means of electric forces.

The remark made already in a footnote of course holds here also. The piezoelectric investigation, as remarked, had shown that change of pressure in the direction of the principal axis did not produce any electricity at the points of pressure. This result seemed worthy of direct proof. For this purpose the plate cut at right angles to the principal axis was provided with a central hemispherical depression just like the plate cut parallel to the axis, and introduced into the flask in the place of the latter; the plates were simply interchanged. Having by this arrangement obtained an interference-band under the depression (that is, at the point where the lines of force run parallel to the principal axis), I was unable to produce any displacement of the band by increasing or decreasing the difference of potential between the electrodes; consequently in this plate also the double refraction was not perceptibly altered by the action of electric forces in the direction of the principal axis. This plate also, upon pressure parallel to the principal axis, gave no piezoelectricity at the points of pressure. If the band were situated at one side of the depression, but very near to it, I observed a phenomenon upon electrification which furnished a very welcome confirmation of the results obtained with the quartz parallelepiped. The lower end of the vertical band remained fixed; but the upper end inclined towards the right or towards the left; and the direction of the motion changed with the sign of the electricity upon the electrodes. Further, I observed that, when the sign of the electricity remained the same, the upper end of a band moved in a different direction according

and a more intense source of light had been employed. If such a change were to be observed, it would certainly be much smaller than that which takes place in the direction of an axis of maximum piezoelectricity. The above experiments would therefore not lose their significance.

as the band was situated to the right or to the left of the centre. Vertical or horizontal compression of the intercalated glass plate produced a displacement of the whole band parallel to itself to the left or to the right.

The explanation of these phenomena is easy to find if we consider that as the experiments described show the direction on the right, so on the left; that is, the horizontal direction at right angles to the rays of light in the crystal employed does not exactly coincide with an axis of no piezoelectricity. The phenomena are thus easily deduced from the first-described experiments:—The lines of force in the upper part of the plate run nearly horizontally in the neighbourhood of the depression; some of them therefore coincide with directions in which the double refraction is capable of being altered: in the lower part of the plate, on the other hand, the lines of force are vertical; they consequently run in the direction of the principal axis, and therefore produce no change of double refraction. Consequently it is only the upper end of the band which is displaced, and not the lower. The observation that the direction of displacement changes when the electrification or the position of the band changes is in complete agreement with the observed fact that the increase of the double refraction of quartz is changed into a decrease if the direction of the lines of force is reversed. We have seen above that from the distribution of piezoelectricity we can predict with certainty, for a given direction of the lines of force, whether an increase or a decrease of the double refraction will take place; and the question consequently arises whether with the new crystal the established rule will be found confirmed or not.

The plate was examined for piezoelectricity. A pressure upon the square surface in the direction of the principal axis produced no distinctly perceptible quantity of electricity at the points of pressure. The four narrow side faces, however, behaved differently; they may be designated *a*, *b*, *c*, *d* in order. An increase of pressure in the direction parallel to *b* and *d* produced positive electricity at *a*, and negative electricity at *c*; a decrease of pressure, electricities of opposite sign. An increase of pressure in the direction parallel to *a* and *c* gave negative electricity at *b*, positive electricity at *d*; a decrease of pressure, the opposite. In both cases I obtained vigorous deflection of the electroscope*. The plate was again

* Between the two directions, parallel to *b* and *d* and parallel to *a* and *c*, there must be a field which in piezoelectric relation would behave oppositely to the two fields to which these directions belong. In fact, an increase of pressure in the direction of the diagonal of the square which joins the corner *a d* with the corner *b c*, gave negative electricity at *a d* and positive electricity at *b c*; a decrease of pressure, the opposite.

placed in the flask full of benzol, and the electro-optical experiments repeated. The optical action of the electricity was also compared with the action of a glass plate compressed in a horizontal or in a vertical direction. In what follows such a note as

“Below +, above —; upper end of band left; horizontal compression”

is to be understood thus:—When the lower electrode was positive and the upper one negative, the upper end of the band lying on the left of the centre inclined towards the side towards which the whole band was displaced by a compression of the glass plate in a horizontal direction.

Experiment I. The rays of light traversed the plate parallel to *a* and *c*; *a* on the left, *c* on the right.

Below +, above —; upper end of band left; vertical compression.

“	—,	“	+	;	“	“	“	horizontal	“
“	+	“	—	;	“	“	right;	horizontal	“
“	—,	“	+	;	“	“	“	vertical	“

Experiment II. Rays of light parallel to *a* and *c*; *a* right, *c* left.

Below +, above —; upper end of band left; horizontal compression.

“	—,	“	+	;	“	“	“	vertical	“
“	+	“	—	;	“	“	right;	vertical	“
“	—,	“	+	;	“	“	“	horizontal	“

Experiment III. Rays of light parallel to *b* and *d*; *b* right, *d* left.

Below +, above —; upper end of band left; vertical compression.

“	—,	“	+	;	“	“	“	horizontal	“
“	+	“	—	;	“	“	right;	horizontal	“
“	—,	“	+	;	“	“	“	vertical	“

Experiment IV. Rays of light parallel to *b* and *d*; *b* left, *d* right.

Below +, above —; upper end of band left; horizontal compression.

“	—,	“	+	;	“	“	“	vertical	“
“	+	“	—	;	“	“	right;	vertical	“
“	—,	“	+	;	“	“	“	horizontal	“

It is easy to convince one's self that these data are in all

respects in complete accordance with the results obtained from the experiments previously described.

It is necessary to mention that the phenomena described above may be explained by means of two known facts. One of these facts has been recently demonstrated by MM. J. and P. Curie*, as follows:—If electricities of opposite sign be communicated to the ends of a secondary axis of a quartz crystal, the crystal contracts or expands in the direction of this axis, according as the signs of the electricities communicated to the ends of the axis are opposed to, or the same as the signs of the piezoelectricities produced at these ends by a pressure exerted in the direction of the axis. It seems to me very probable that this result, found in the first instance for the direction of a secondary axis, would be found to hold good also for every direction at right angles to the principal axis, and that consequently the direction of the intermediate axis or the axis of no piezoelectricity possesses the property that electrical forces acting in this direction produce no perceptible change in the form of the quartz. So far I have had no opportunity to test the accuracy of Curie's experiment, and to extend it in the direction indicated; but inasmuch as those experiments interest me very much because of the close relationship in which they stand to my former experiments on the so-called electrical expansion†, I shall undertake this investigation at the earliest possible opportunity.

The second fact, admitting of easy confirmation, is that a mechanical compression of quartz at right angles to the principal axis exerts the same qualitative effect upon the rays of light which traverse the crystal at right angles to the principal axis and to the direction of compression as a compression exerted in the same direction upon an interposed plate of glass.

We shall find without difficulty that the phenomena described are even in detail completely in agreement with the properties of quartz just now described.

I hope shortly to give an account of the remarkable phenomena which I have observed when the rays of light traverse the crystal parallel to the principal axis.

Giessen, November 25, 1882.

* *Compt. Rend.* vol. xciii. p. 1137 (1881).

† *Ber. d. Oberh. Ges.* vol. xx. p. 1 (1881).

XX. *Notices respecting New Books.*

Madeira Spectroscopic, 1881-82. By C. PIAZZI SMYTH, *Astronomer Royal for Scotland*. W. and A. K. Johnston : Edinburgh.

THIS volume, read in connexion with 'Madeira Meteorologic' (D. Douglas, Edinburgh) by the same author, forms a valuable contribution to the moist-climate history of that island; but its more immediate object (as we read on the title-page) was "the revision of 21 places in the red half of the solar visible spectrum with a Rutherford diffraction-grating at Madeira in the summer of 1881."

This is effected in 32 closely printed large quarto pages, and 17 photo-lithographed plates. At first sight the latter rather shock the eye by certain dense black columns of irregular heights and masses of apparently coarse shading; but by and by we ascertain how this arises.

Part I. opens by explaining the local disadvantages presented by the Royal Observatory at Edinburgh for such work, and the need of a station where the sun was nearer the zenith. Madeira, though offering one day only in ten of pure blue sky (but that one of splendid quality), was selected, and at the Quinto da Corvalho, near Funchal, a heliostat and spectroscope were erected. The sun's beam from the heliostat was transmitted through a 6-inch achromatic lens of long focus to a collimator of 31 inches focal length, then reflected from a Rutherford grating 1.6 inch square with 17,296 lines to the inch, and finally viewed with a telescope of 52 inches focal length. The lenses of the collimator and telescope were respectively of quartz, 2.25 inches in diameter; and eye-pieces were used magnifying 30, 45, and 64 times upon the second or third order of spectrum.

In front of the slit a white flint-prism separated the colours for the particular portions of the spectrum examined. This fine piece of apparatus secured the author his Madeiran spectra, drawn on a nearly uniform scale of wave-numbers to six places of figures.

The earlier Solar Maps with which these are compared are thus enumerated:—

(1) Sir David Brewster and Dr. Gladstone's in *Phil. Trans.* 1860. A careful copperplate, but on too small a scale, except as to certain magnified groups: a prismatic spectrum.

(2) Prof. Ångström's normal (grating) solar spectrum, 1868. Most exemplary, on stone with "incised" lines.

(3) Kirchhoff and Hofmann's prismatic spectrum as presented in Roscoe's 'Spectrum Analysis' (1869). Defective, owing to errors of "registration" in the four tint stones used in the printing.

(4) From Dr. Schellen's 'Spectrum Analysis:' a print of the last map from wood-engraving, liable to similar errors.

(5) Rutherford's single stone lithograph (about 1874), from his fine photograph of the blue and violet half of a prismatic spectrum;

discriminating and faithful in reproducing lines, bands, shadows, &c.

(6) Janssen's copperplate prismatic spectrum from C to D (about 1874). Very accurate, almost microscopic.

(7) The Royal Society's prismatic second Himalaya spectrum, by Hennessey, in Phil. Trans. 1875. On stone, mechanical but certain, with the merit of showing high- and low-sun spectrum from the ultra-red to Great F.

(8) Captain Abney's drawing from his remarkable photographs of the ultra-red region beyond the range of optical vision (grating), 1878.

(9) Prof. Langley's lithographic views of Great A and Great B (1878).

(10) M. Fievez's engraving on stone of a grating-spectrum of the little "b" group (1880).

(11) Prof. Vogel's Potsdam prismatic-spectrum map from near E to K, on three times the scale of Ångström's map, part based on eye-observation and part on photographs (1880).

(12) Lastly, Prof. Young's pencil-sketch of the Great E group, on nine times the scale of Ångström's normal map. A most refined and exact drawing from a fine grating (1881).

Later on (1882) the author received a copy of M. Fievez's grand "Spectre Solaire" (obtained with a Rutherford grating and two Christie half-prisms), and substituted its results for some of his Madeiran duplicates.

Part II. opens by describing the frontispiece plate (0) as containing three spectrum-scales, the British inch being used in these and the other spectrum-plates as the unit. Form 1 is a wave-number shape, with about equal portions of the red and violet ends. Form 2 a wave-length shape diffraction (grating) spectrum, with much red and a contracted violet. Form 3 an average refraction (prism) spectrum, showing a contracted red and opened-out violet.

We now proceed to the 21 subjects and their 147 examples, which we can but notice briefly, though each of them contains some interesting, and often novel, feature.

Plate I., subject 1, Great A. We first get an explanation that a perfect reproduction of the exquisite lines and shadows seen in the solar spectrum is difficult to impossibility, and a pointing-out that the shading by *vertical* lines generally adopted by lithographers and engravers is mischievous and deceiving.

Prof. Smyth then lays down as *rules* :—

1st. Any vertical line shall represent a true spectrum-line and nothing else.

2nd. Greater or less thickness of lines shall be represented truly ; but degrees of intensity by shortening the height of column, or (less easily) by dotting, stippling, or crossline.

3rd. Thin hazy lines by vertically dotted or vertical linearly wavy lines ; or such *shorter* figurings in ink as would, if smeared vertically, produce a correspondingly pale line.

4th. Broad and pale lines and shadings in their proper breadths, but produced by close thin lines drawn in *any direction except the vertical*.

These are the rigid principles on which the spectra are constructed in the author's plates. An example *per contra*, by way of warning, is adduced from Brewster and Gladstone's map (example I.), where every line in the Great-A band has been evidently doubled, not by the observers but by the engraver.

On the whole therefore the author decided to magnify all the old drawings to his own scale; and to *symbolize* their shadings, giving dates and the number of times linear of enlargement. Hence, and from the small original scale of some of the maps, come the "cyclopean" columns in some of the examples.

Great A and its preliminary band take up two plates and 14 examples (subjects 1 and 2). The beautiful splitting up of the preliminary band into a rhythmical series of double lines is well shown in the Madeiran examples. The Royal Society's spectrum is represented by a blur of shade for the band and an immensely broad line for Great A, showing no splitting-up whatever, and in this respect is in no way equal to Brewster and Gladstone's much earlier drawing.

As regards Great A itself, Prof. Langley's spectrum alone seems to compare in number of lines shown with the two Madeiran examples.

While discussing Great A, the author points out the need of a fine slit and accurate focus to bring out the doubling of the lines of the preliminary band. He considers that the origin of these lines, if telluric, is not watery vapour, but some dry gas in the upper regions of our atmosphere, or in the millions of miles of space beyond, and suggests a mode of research "by looking through 500-feet long metal tubes with glazed ends filled in succession with every known gas, at a light of constant quality."

"Little a" and its band of lines form subject 3. This line and group were ascertained by Angström to be caused by invisible watery vapour in the lower part of the earth's atmosphere.

The earlier spectra fairly represent the object, except that we are told that, in the Royal Society's plate, vertical "engraver's" shading-lines mislead. The two Madeiran spectra show a great number of lines; and we find (example 5) a very interesting Lisbon spectrum of the author's, with high sun and in a dry air, in which the lines become very fine. On page 10 we have a suggestion that on the Peak of Teneriffe they would possibly cease to have an apparent existence in the solar spectrum.

Great B with its preliminary and attached bands, as seen in a high and low sun respectively (subjects 4, 5, 6, and 7), occupies 3 plates and 28 examples. Here (preliminary band) the earlier map of Angström's, as respects double lines in rhythmical series, contrasts favourably with the later maps until we come to the Madeiran views. The author discusses whether one of Prof. Langley's lines is, or is not, outside a couplet in the series; and records

the important observation that a filling-up of close parallel lines is a failing peculiar to spectroscopic vision, and that such close lines throw out a haze towards each other, thatching-in the intervening space. Great B with its attached band shows more distinctly the excellence and value of the author's observations. The earlier masses of almost black shadow (in the Royal Society's spectra quite so) are broken up by the author's apparatus into thin lines.

Subjects 8 and 9, with 4 plates and 14 examples, introduce us to Great C, the first in order hydrogen-line. The principal point noticed here is the hazy and pale character of this important line, as seen in the Madeiran spectra, when compared with the blacker and sharper aspect presented in the comparison spectra. The author explains this by reminding us that the observation of this line results from the integration of all the hydrogen activities going on over all the sun's surface, and that consequently the line C could have no definite outline; and he gives adequate reasons for the black and well defined lines of observers with less powerful instruments.

Subjects 10 and 11 with Plates IX. and X. illustrate "little α " (alpha), or C6 band; and again we find the author's spectra showing a series of fine lines but sparingly represented in the earlier maps, and of which (in a high sun) three only appear in the Royal Society's spectrum. The high- and low-sun views of this object differ essentially in strength, but not in general character.

The indications of telluric gas-bands in nature's *cold* way, the question of place of a red aurora-line, and of a gold line falling on the very centre of intensity of this band, are referred to by the author.

Subjects 12 and 13, Plate XI., are the D or sodium-lines, so well known to all spectroscopists. Ångström's map shows one (the Ni) with a high sun, and five with a low sun, of intermediate lines, with the D lines black and sharp in both cases. The improvement in modern observation and instrumental power is evidenced in the Madeiran examples by five intermediate lines with a high sun, and ten with a low. Notably also the Madeiran examples show the D lines comparatively pale and fuzzy (though not to the same extent as Great C), demonstrating that solar sodium (Na) enters in part into the composition of the prominences. The intermediate and adjacent fine lines to D¹ and D² are proved to be due to telluric watery vapour; and it is shown that with a really good spectroscope there need be no undefined haze about them.

From D we pass to Helium, 47,778 W. N. Pl., subject 14, Plate XII. The history of this line is given, and its observation in the eclipses of 1870, 1871, and 1872 is referred to, together with its presumed (but now disproved) detection in the aurora and zodiacal-light spectra. In Ångström's map it appears as a single line marked only Fe. Roscoe makes it single. The Royal Society's map ignores it altogether. Vogel doubles it, but doubtfully, "as it may be a tint-stone line sticking out on one side." The Madeiran spectrum beautifully and sharply doubles it; and, lastly, M. Fievez's

"spectre solaire" shows it as a wide hazy filled-up double. We note here the widening and filling-up of M. Fievez's doubles; and on page 17 are curious instances of the fact that spectroscopic power does not necessarily imply fine definition. Where Prof. Young with a single Rutherford grating saw 27 thin lines, M. Fievez, with a similar grating and two Christie half-prisms added, saw only 13. Prof. Young has also one more strong line split into two than M. Fievez; and in matter of doubles shown to be triples, the former has one which the latter has not.

Subjects 16 and 17, Plates XIII. and XIV., relate respectively to the bandelet of lines following E, and to a "basic" line preceding "little b." In the latter subject we again find a single line of Ångström's, of Kirchhoff and Hofmann's, and even of Vogel's map, beautifully split by our author and by M. Fievez, the line itself being left to imagination in the Royal Society's map.

The "little b" (magnesium) group comes next, in subject 18, Plate XV., with a repetition in b^1 , b^2 , and b^4 of the pale and hazy effects shown in Great C and D (indicative of the volatility of magnesium to solar heat) in Vogel's, the author's, and M. Fievez's maps. b^4 , a single line by Ångström attributed to Mg and Fe, remains single in the earlier maps, including Vogel's, but is well divided by the author and M. Fievez; while the latter seems also to have divided b^3 , marked Ni and Fe by Ångström, and retained as single even in the Madeiran spectrum. In Plate XVI., subject 19, a group following "little b," we get three basic lines of Ångström's, all marked Fe and Ni, neatly divided by the author and M. Fievez, they making a triple of one of Vogel's doubles.

Plate XVI., subject 20, is Great F, with the paleness and haze characteristic of a solar storm-line (duly recorded, by the way, by Ångström). The comparative effect of eye and photographic brightness as between the *glaucous* and *violet* hydrogen-lines is here discussed.

"Near G" line or violet hydrogen, Plate XVII., subject 21, ends the typical series with further illustrations of the hydrogen haze and paleness.

Example 4 on Plate XVII. gives us the interesting part of the spectrum adjoining "near G" as shown in Rutherford's photograph; and it is noteworthy how closely this absolute reproduction corresponds with the Madeiran hand-drawn spectra.

If we were æsthetically startled by the black and white of Plates I. to XVII., Plate XVIII. effectually consoles us; for, without exception, we have seen no specimen of chromo-lithography to compare with it for brilliancy and blending of tints. It does the greatest credit alike to the author's artistic conception and to the lithographer's skill. It represents three diagrams of colours on spectrum principles:—(1) Pure or single spectrum colours in their natural order; (2) mixed or double spectrum colours; and (3) very mixed or treble spectrum colours. The diagrams most

effectively show gradations of light and shade and differences of colour-distances, while in the fine cross-hatching, which delicately indicates shading, the vertical line is carefully avoided. The only sections where we thought gradation was not quite perfect are deep blue (58,000) and grey (67,000); otherwise the plate seems to be real perfection in a very difficult subject.

A special note requests that if these colour-diagrams are used for star-colour comparison in observatories (for which they seem very appropriate), they may be viewed by a white light, a small Swan lamp being preferred.

Pages 25 to 28 comprise the author's summary of his Madeiran results; and with a genuine lament at the ignorance we are all in of the chemical character of Great A, Great B, and the Alpha Band (as evinced at the discussions of the British Association at Southampton, 1882), he gives us his own views of it, to which we must refer the reader. An Appendix prints Prof. Josiah P. Cooke's valuable paper on the aqueous lines of the solar spectrum (Proc. Amer. Acad. of Boston, vol. vii. p. 57); and a pretty vignette from a sketch by the author adds the ornamental to the useful.

That so elaborate and precise a work should have been carried out by one hand, and under certain drawbacks to which Prof. Smyth feelingly alludes, is not one of the least of the merits. To be able to trace the recorded history of typical solar lines and groups, with a final examination of them as shown in an almost *ne plus ultra* instrument, and under splendid air-conditions hardly attainable in this country, is no slight gain to spectroscopists. To these in particular, as to scientists in general, 'Madeira Spectroscopic,' cannot fail to prove a standard work of the highest value.

XXI. *Intelligence and Miscellaneous Articles.*

ON AN ELECTRODYNAMIC METHOD FOR THE DETERMINATION OF THE OHM. THE EXPERIMENTAL MEASUREMENT OF THE CONSTANT OF A LONG INDUCTION-COIL. BY G. LIPPMANN.

THE electromotive force employed in this method is produced by the relative displacement of two circuits, as in the well-known experiment of M. Kirchhoff. The entire arrangement is nearly the same as in the method of M. Lorenz.

1. A movable frame rotates about one of its diameters with a uniform velocity of n turns per second. It is placed inside a fixed coil traversed by a current of intensity i , which at the same time traverses the wire of which the resistance is sought. The induced circuit is closed only for an instant, at the moment when the electromotive force passes through its maximum value e . At that moment it is opposed to the difference of potentials which arises between two points A and B on the wire, chosen so that there is

equilibrium: the equilibrium is ascertained by means of a sensitive galvanoscope. If r be the resistance of the portion of the wire comprised between the straight sections passing through A and B, S the surface covered by the induced wire, C a constant peculiar to the coils employed, we have the following condition of equilibrium, which at the same time gives the value of r :—

$$v = 2\pi nCS.$$

The employment of a frame carrying p turns of wire gives electromotive forces p times as great as those which would be obtained if Lorenz's disk were used; consequently the effect of thermoelectric perturbations becomes negligible.

It is known that the determination of C is too complicated for its approximation to be easily indicated. I believe that calculation may be altogether dispensed with by operating in the following manner:—If a fixed coil were employed of infinite length in proportion to its diameter, C would be known: we should have, exactly,

$$C = \frac{4\pi}{d}, \text{ } d \text{ being the mean distance between two turns of the wire.}$$

Now we cannot construct an infinitely long coil; but an equivalent result can be obtained. The movable frame is first placed in the centre of a fixed coil of which the length is, for instance, 2 metres, and the points A and B are obtained as was said above. Then, the movable frame remaining in its place, the fixed coil is brought into a second position, which is the extension of the first: a new interval BB' is thus obtained on the wire, immediately following AB ; the small additive segment BB' is the lengthening undergone by AB when the inducing coil is lengthened 2 metres. We can thus lengthen indefinitely the inducing coil by simply displacing one and the same segment; and we very quickly arrive at segments which can be neglected in comparison with AB . The determination is then finished.

It will be remarked that this method is one of the most direct; it requires no calculation for reduction or correction. If the derivation-points, such as A and B, are needle-points, the final distance between them is the final result sought, without any correction. It follows that the control of the method is equally direct.

2. The above-explained way to determine C experimentally can be applied to other problems besides the construction of the ohm; it can be employed especially for constructing a tangent-compass or an absolute dynamometer. For this purpose, either a movable magnet or a movable coil is placed in the centre of a fixed coil.

The constant of the instrument is equal to $\frac{4\pi}{d} - \delta$, δ being the sum of the moments of the deflections obtained on successively removing the fixed coil further and further until it reaches infinite distance.—*Comptes Rendus de l'Académie des Sciences*, Dec. 26, 1882, t. xcv. pp. 1348–1350.

AMOUNT OF CARBON DIOXIDE IN THE ATMOSPHERE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

My thanks are due and tendered to Prof. John LeConte for pointing out a numerical error relating to the above which has crept into my paper on "Carbon Dioxide as a Constituent of the Atmosphere," published in the number of your Journal for November last. It is pleasing to be able to add, however, that the main conclusions and arguments of that paper are not thereby affected. The mistake arose apparently by making use in some unaccountable manner of the formula for a prolate spheroid instead of that for an oblate one. Making the correction and recalculating on the basis of 3 vols. of CO_2 per 10,000 (which all recent investigations show to be nearer the truth than 4), we obtain

Weight of CO_2 in atmosphere = 2420 billions of kilogrammes nearly.

The great difference between this number and that given by the older chemists and stated in text-books will, I think, justify the conclusion drawn in my paper, that the generally received idea of the amount of CO_2 in the atmosphere is much too great.

I am, Gentlemen,

Your obedient servant,

ERNEST H. COOK.

EXPERIMENTS ON THE DIRECT PHOTOGRAPHY OF SOUND-VIBRATIONS. BY PROF. BOLTZMANN.

A small thin platinum plate was attached perpendicularly to the centre of a thin iron plate, which, as in the telephone or phonograph, was fixed on a wall-piece. It was first ascertained that the small platinum plate really repeated approximately unchanged the vibrations of the sound that arrived in the capsule. For this purpose a second small platinum plate was fixed immovably in the vicinity of the first. The resulting fine slit between the two was brought into the focus of a collecting-lens, upon which sunlight fell. After passing through the slit, the rays reached a Breguet selenium cell, which, together with two telephones, was inserted in the closing circuit of twelve Leclanché cells. Single sounds and words spoken into the mouthpiece could be heard most distinctly in the telephones. When the rays, after issuing from the slit, were rendered as nearly as possible parallel, and intercepted at a greater distance by a large collecting-lens to be concentrated upon the selenium cell, the apparatus could also be employed as a photophone.

After these preliminary experiments, intense sunlight was again concentrated upon the vibrating platinum plate; and then, by

means of a solar microscope, an image of the shadow of the platinum plate was thrown upon a screen. The bounding line of the shadow, as nearly as possible straight, was condensed by a cylindrical lens. In the place of the screen a glass plate, prepared with Vogel's emulsion, was now moved rapidly across by a strong spring, so that the direction of its motion was perpendicular to the line of light produced by the cylindrical lens, while the mouthpiece was spoken into. Side light being duly kept off, a bounding line between light and shadow was then obtained on the prepared plate, forming a curve corresponding to the sonorous vibrations. To the vowels pretty simple curves correspond, often approximately curves of sines, often interference-curves of two or three curves of sines. With the vowel *a* a period contains the greatest, with the vowel *u* the least number of indentations. To the consonants *l*, *m*, *n*, *r*, and also especially *p* and *k*, uncommonly multifarious curves correspond, having a resemblance to those found by König for *r* by means of his tone-flame, but showing in addition much finer details.

The author intends to repeat the experiments by photography upon rotating disks in order to be able to take up a greater number of successive vibrations.—*Kaiserl. Akad. der Wissensch. in Wien, math.-naturw. Classe*, Nov. 30, 1882.

ON CENTRAL FORCES AND THE CONSERVATION OF ENERGY.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Permit me to point out an error in Mr. Browne's paper with the above title in the January number of the *Philosophical Magazine*. On page 37 he says, "Then it is easy to show that *F* must not vary with θ . For if it does, let us suppose that when *B* has come to rest, and before it is allowed to return, it is made to rotate about *A* through an angle $d\theta$, and again brought to rest. Then the circumstances of *A* and *B* are unchanged." This last statement is only true if *F*, the component of the force in the direction *AB*, constitutes the whole force; for otherwise work will be done during the rotation, and therefore the potential energy of the system will be changed. Thus Mr. Browne's conclusion that "the force with which *A* acts upon *B* always tends towards *A*," is tacitly assumed in the proof. It therefore appears to me that the only conclusion which Mr. Browne is entitled to draw is that *if* the force with which *A* acts upon *B* always tends towards *A*, then its magnitude must be a function of the distance only.

I have the honour to remain, Gentlemen,

Your obedient servant,

G. W. VON TUNZELMANN,
H.M.S. 'Britannia,' Dartmouth.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
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JOURNAL OF SCIENCE.

[FIFTH SERIES.]

MARCH 1883.

XXII. *The Selective Absorption of Solar Energy.*
By S. P. LANGLEY*.

[Plate III.]

Introduction.

IN 1800 Sir William Herschel published his remarkable investigations, in the *Philosophical Transactions*, on obscure heat, in which he reached the conclusion that heat is an entity distinct from light.

This view was modified by subsequent writers into the statement that each ray has three qualities—heat, light, and chemical action; while at the present time many physicists have reached the further conception that heat, light, and chemical action are not so much qualities inherent in any ray, as modes of the manifestation of one common energy—or, to state this view in the broadest manner, that the same æthereal wave will give us either heat, light, or chemical action, according to the absorbent nature of the substance which receives it.

These last opinions, however, cannot be said even yet to be universally accepted by physicists.

Dr. J. W. Draper long since pointed out that the maximum of heat did not necessarily lie in every case below the red (though it does so in the prismatic spectrum), and that in a normal spectrum it would be in the orange. These conclu-

* Communicated by the Author, to whom we are also indebted for the clichés of the illustrations.

sions as to the normal spectrum were unverifiable by direct experiment by any means he possessed. He found it impossible, with the most delicate thermopiles at his command, to get sensible heat from the grating-spectrum without gathering all that lay in the two halves of it together, and could consequently only infer the result of complete measures. But it followed that, if it ever became possible to measure amounts of heat so minute, these conclusions could be verified on separate rays of the diffraction spectrum.

No one, so far as I know, has hitherto succeeded in measuring the heat from a diffraction-grating except in the gross—by thus concentrating, for instance, the whole upper half and the whole lower half of its spectrum upon the pile, and so reaching some results not without value, even as thus obtained, but of quite other interest than those which may be expected when we become able to measure with close approximation the separate energy of each wave-length. Having devoted many years to the study of the solar radiant heat by means of the thermopile, I was led to hope that, by my long apprenticeship to the precautions needed with this instrument and the possession of the most delicate apparatus attainable, I might succeed better than my predecessors. I found, however, that though I got results, they were too obscure to be of any great value, and that science possessed no instrument which could deal successfully with quantities of radiant heat so minute; for the average heat in the diffraction spectrum does not, under the most favourable circumstances, reach one tenth that in the prismatic one, and is usually much less even than this.

Impelled by the pressure of this actual necessity, I therefore tried to invent something more sensitive than the thermopile, which should be at the same time equally accurate—which should, I mean, be essentially a “*meter*” and not a mere *indicator* of the presence of feeble radiation, and was led by nearly a year’s continual experimenting to the construction of the Bolometer (*βολή μέτρον*), an instrument the details of whose construction are described in the Proceedings of the American Academy of Arts and Sciences, vol. xvi. (1881). With this apparatus the experiments on the diffraction spectrum were resumed—the first entirely unquestionable evidence of measurable heat, in a width so small as to be properly described as linear, having been obtained on October 7, 1880. Nearly the whole year 1880 passed in modifications of the instrument, or in the making of those measures which gave promise from the first of bringing results of value.

It will be seen that they afford almost all the experimental

evidence the subject admits of; that every ray, whether lying in the "chemical," "visible," or "heat" region, is capable of making itself known as heat, and that the maximum of heat in the normal spectrum is near the yellow.

Further, by taking all the observations twice daily, at times when the atmospheric absorption is very different, we are able to calculate (for the first time) the amount of this absorption for each separate spectral ray. These researches are necessarily long and difficult; but they have led to some very unexpected results. The reader who wishes to pass at once to these results will find them in the summary at the close of this memoir.

Preliminary Observations.

The measurements with the grating possess the inestimable advantage of enabling us to fix the wave-length of every ray measured; but while the heat in the grating-spectrum is, as has just been said, at best less than one tenth that in the prismatic, the latter is itself, when taken in portions so narrow as to be approximately homogeneous, almost insensible. The difficulties of measuring heat with the grating are greatly complicated by the overlapping of the spectra. In these first measures, which were carried to a wave-length of one thousandth of a millimetre*, I have employed two of the admirable gratings of Mr. Rutherford—one containing 17,296 lines to the inch, or 681 to the millimetre, and one half that number, both ruled upon speculum-metal.

I have used a slit at a distance of 5 metres, without any collimator, and have kept the grating normal to the optical axis. It will be seen, then, that the rays have passed through no absorbing medium whatsoever, except the sun's atmosphere and our own.

The rays from the grating fall upon a concave speculum (whose principal focal distance is about 1 metre), and from this are concentrated upon the mouth of the bolometer, forming a narrow spectrum, which passes down the case of the instrument and falls upon the bolometer-thread. As this thread moves along the spectrum parallel to the Fraunhofer lines, its coincidence with one of them is notified by a lowering of its temperature and a deflection of the galvanometer. The instrument is of course equally sensitive to the invisible radiation as to the visible. It is important to observe that no

* Throughout these measures the unit of wave-length will be the *micron* (μ) = $\frac{1}{1000}$ millimetre, or 10,000 times the unit of Ångström. Thus the wave-length of Fraunhofer's "A" is here written 0 μ ·76.

screen is interposed between the bolometer and the grating ; for the temperature of the screen itself, as it is replaced or withdrawn, will certainly affect such measurements as these. Through the whole course of the experiment the bolometer is uninterruptedly exposed to radiations from the grating, whether reflected by it or emanating from its own substance. The interruption of the solar radiation is effected at the other end of the train, 5 metres beyond the grating itself. In the gratings employed one of the second spectra is very feeble, or almost lacking. The rays of the second spectrum are necessarily superposed on those of double the wave-length in the first ; and as all evidence of solar radiation in the most sensitive apparatus at the sea-level dies out near $\lambda = 0^{\mu} \cdot 3$ in the ultra-violet, it follows that we can measure down in the first spectrum as far as $\lambda = 0^{\mu} \cdot 6$, or in fact further, without any fear whatever of our results being affected by the underlying second spectrum, even if that were a strong one. Underlying $0^{\mu} \cdot 7$, which is near the limit of the red in the first spectrum, is $0^{\mu} \cdot 35$ in the second, where heat is practically still negligible. We have therefore (knowing the amount of heat in the second spectrum at $0^{\mu} \cdot 5$, and knowing that our ultimate point of measurement at $1^{\mu} \cdot 0$ in the first spectrum overlies $0^{\mu} \cdot 5$ in the second) the means of asserting with confidence that no considerable error can be introduced from this cause, after an allowance has been made here for the minute effect of this second spectrum. An allowance is also made to reduce the effect to that which would have been observed with a grating so coarsely ruled as to cause no considerable deviation from the slit of any portion of the spectrum measured. The bolometer (in a constant position relative to the concave mirror such that the optical axis of the latter bisected the angle between its central thread and the centre of the grating) was moved, together with the mirror, by a tangent-screw in arc, so that the spectrum appeared to traverse its face.

The actual angular deviation of any ray under examination was obtained from a divided circle on which the arm carrying both mirror and bolometer moved. A particular description is not given, as the whole apparatus was replaced by a more perfect one later. That actually used will be intelligible by the sketch, fig. 1, where S is the slit, G the grating, M the concave mirror, B the bolometer, and C the divided circle.

The light came from the silvered mirror of a heliostat, passing through the slit at a distance of about 5 metres from the grating, which was bolted immovably above the centre of the circle of a massive dividing-engine, with the grating's

plane always perpendicular to the line joining its centre and the slit. The mirror and the bolometer, with their attachments, were fastened to this movable circle.

An allowance has been made for the absorption of speculum-metal and silver; but the absorption of the iron strips of the bolometer has only been indirectly allowed for. This has been done by comparison with the action of a bolometer with lampblack surface. It will be seen hereafter that the whole experiment has been repeated with a lampblack bolometer without in any way affecting the results. The wave-lengths are derived from the measured angles by the use of the formula $ns\lambda = \sin i + \sin r$, where n is the order of the spectrum, s the space between the lines of the grating, λ the wave-length of the radiation, i the angle of incidence (in the present instance 0°), and r the angle of diffraction.

In the early observations it appeared from the examination of the diffraction-spectrum up to $\lambda = 1^{\mu}0$ that the energy in the invisible part as far as this was much less than in the visible. Nothing definite is even at this time known to physicists as to the extent of the normal solar spectrum; but the prismatic spectrum is still very commonly supposed to be limited by theoretical considerations to an extent little greater than this; and one of those most conversant with the subject has treated this wave-length (*i. e.* $1^{\mu}0$) as marking the limit of everything known to exist*.

Fig. 1.



* Draper, "On the Phosphorograph of a Solar Spectrum, and on the Lines in the Infra-red Region," *Philosophical Magazine*, vol. xi. p. 167, March 1881. He asks, "Do we not encounter the objection that this wave-length, $10 \cdot 750^{-10}$ metre (the limit of Captain Abney's map), is altogether beyond the theoretical limit of the prismatic spectrum?" Previous measurements of heat had, it will be remembered, been made by comparing its total amounts in the visible and invisible prismatic spectrum, which gives us no knowledge as to wave-lengths in any case; and

It seemed at first, then, improbable that the heat below the red should materially exceed or even equal that above it; for this would demand (since the heat shown by the last ordinate at $\lambda = 1^{\mu} \cdot 0$ is very small) an extension of the curve of heat, as obtained from the grating, to a distance enormously beyond the furthest limit then assigned to the normal spectrum by experiment. The writer's further investigations, however, led him to believe that this immense and unverified extension has really existed, and to thus confirm by independent means the statements of Tyndall and others as to the great heat in this region. He was unable to determine its exact limit with the grating as then used, on account of the overlapping spectra, but was, some two years since, led, from experiments not here detailed, to suspect the existence of solar heat at a distance of nearly four times the wave-length of the lowest visible line, $A(\lambda = 0^{\mu} \cdot 76)$ or at $\lambda = 3^{\mu} \cdot 0$.

We receive all the solar radiations through an absorbing atmosphere; and it is of the first consequence to determine the rate of this selective absorption for each separate ray. This has (owing to the difficulties before alluded to) never been, so far as I know, yet attempted. It forms a prominent part of the present design.

The great difficulty in this investigation, after the provision of a sufficiently delicate heat-measurer, lies in the varying amount of radiant energy which our atmosphere transmits, even for equal air-masses. The solar radiation is itself sensibly constant; but the variations in the radiant heat actually transmitted are notable, even from one minute to another under an apparently clear sky. The bolometer, in fact, constantly sees (if I may use the expression) clouds which the eye does not. That these incessant variations are in fact due to extraneous causes and not to the instrument itself, has been abundantly demonstrated by measurements on a constant source of heat.

Those taken, for instance, on a petroleum-lamp, so placed as to give nearly the same galvanometer-deflection as the sun

wave-lengths in the dark-heat region had been estimated by hazardous extrapolations from contradictory formulæ—formulæ which profess a theoretical basis, but contradict each other. Thus Müller finds by Redtenbacher's formula a wave-length of nearly $5^{\mu} \cdot 0$ for the extreme solar heat-rays, Draper (as we have just seen) a wave-length of but $1^{\mu} \cdot 0$ for the same rays, &c. All these formulæ (Briot's, Cauchy's, &c.) agree well with the observations in the visible spectrum, which they have in fact been originally deduced from. They contradict each other thus grossly when used for extrapolating the place of the extreme infra-red rays, whose real place we give later from actual measures.

did, were found to indicate a probable error, for a single observation, of less than 1 per cent.

The variations from minute to minute (under a visually clear sky) amount frequently to ten times the probable instrumental error; and they can only be eliminated by repeating the observations a great number of times on many different days. Actually twenty-nine such days' observations have been made (as appears below) in the preliminary series; but it would be an error to suppose that this number was obtained without the sacrifice of a still larger number on which the apparatus was prepared and the day spent without results, owing to the still more considerable atmospheric changes between morning and afternoon. Even of the twenty-nine days cited, and which may be considered exceptionally fair, it will be seen that in only ten cases did the sky continue sufficiently constant in the morning and afternoon to allow complete series to be taken.

It will be understood that we aim to make at least two sets of measures throughout the spectrum daily—one when the rays have been little absorbed (at noon), the other when they have been greatly absorbed (in the morning or afternoon). The mass of air through which the rays pass is taken proportional to secant ζ for zenith-distances less than 65° , and for those greater to $\frac{.0174 \text{ tabular refraction}}{\cos \text{ apparent altitude}}$, and in both cases to the actual barometric pressure. It is expressed in units, each of which is represented by the pressure of one decimetre of mercury at the sea-level. As the barometric pressure there is 7.6 decim., $t^{.6}$ gives the transmission for an entire atmosphere. The coefficient of transmission, then, for one atmosphere ($t^{.6}$) is the proportion of the radiation transmitted by a sun in the zenith to an observer at the sea-level; and this is here shown to vary greatly for each ray. Thus, by reference to Table III., we find of three solar rays whose wave-lengths are .375, .600, 1.000, that of the ray whose wave-length is $0^{\mu}.375$ (in the ultra-violet), 61 per cent. of the original energy would be absorbed and 39 transmitted; of wave-length $0^{\mu}.600$ (in the orange), 36 per cent. would be absorbed and 64 transmitted; of wave-length $1^{\mu}.000$ (in the infra-red), 20 per cent. is absorbed and 80 transmitted, &c.

Allegheny Bolometer Observations on the Solar Diffraction-Spectrum previous to Mt. Whitney Expedition.

The following list shows the dates at which bolometer observations were made at Allegheny up to June 1881, for the measurement of heat in the spectrum and the determination of

atmospheric transmission, by the comparison of the noon and afternoon measures. Those days on which noon measurements were taken which were rendered useless for this purpose, by subsequent changes in the condition of the sky or by other causes, are indicated by an asterisk. It will be seen that of twenty-nine days of observation only ten observations could be fully utilized.

Dates:—1880, Nov. 12,* Dec. 11,* Dec. 18.* 1881, Jan. 12,* Jan. 18,* Jan. 28, Feb. 2, Feb. 3,* Feb. 5,* Feb. 17, Feb. 19,* Feb. 22,* Feb. 26,* March 2,* March 10,* March 11,* March 25,* March 28,* April 7,* April 16,* April 22, April 23, April 28*, April 29, April 30, May 4,* May 26,* May 27,* May 28.

The following table gives the observed galvanometer-deflections reduced to a scale on which the readings are proportional to the current passing through the galvanometer.

d_i = galvanometer-deflection with high sun,
 d_{ii} = " " " low sun.

TABLE I.

$\lambda =$		0 μ ·375	·400	·450	·500	·600	·700	·800	·900	1·000
Jan. 28, 1881...	$\left\{ \begin{array}{l} d_i \\ d_{ii} \end{array} \right.$...	101	...	374	383	320	221	144	102
		...	43	...	167	268	215	221	116	78
Feb. 2.....	$\left\{ \begin{array}{l} d_i \\ d_{ii} \end{array} \right.$	34	80	215	289	307	293	175	...	93
		3	20	61	104	141	195	91	...	47
Feb. 17	$\left\{ \begin{array}{l} d_i \\ d_{ii} \end{array} \right.$	23	62	120	232	260	227	188	...	71
		8	25	58	110	133	151	80	...	39
April 22	$\left\{ \begin{array}{l} d_i \\ d_{ii} \end{array} \right.$	19	43·5	154	236	262	239·5	177·5	123·5	98
		6·5	17	63	119·5	171·5	180·5	122·5	89·5	84
April 23, A.M....	$\left\{ \begin{array}{l} d_i \\ d_{ii} \end{array} \right.$...	59	152	206	263	227	191	121	94
		...	41	124	189	258	257	187	122	96
April 23, P.M....	$\left\{ \begin{array}{l} d_i \\ d_{ii} \end{array} \right.$...	59	152	206	263	277	191	121	94
		...	32	103	124	188	198	140	80	66
April 29, A.M....	$\left\{ \begin{array}{l} d_i \\ d_{ii} \end{array} \right.$	13	29	113	151	235	235	139	100	89
		10	22	65	126	156	197	135	93	86
April 29, P.M....	$\left\{ \begin{array}{l} d_i \\ d_{ii} \end{array} \right.$	13	29	113	151	235	235	139	100	89
		5	8	49	62	107	116	72	58	66
April 30.....	$\left\{ \begin{array}{l} d_i \\ d_{ii} \end{array} \right.$	21	55	121	186	245	259	175	119	90
		18	33	97	148	220	232	166	97	80
May 28	$\left\{ \begin{array}{l} d_i \\ d_{ii} \end{array} \right.$	8	34	99	109	144	134	89	64	52
		...	2	9	27	52	66	61	33	39

The next table gives the sun's position, and the corresponding air-mass for each series in the previous table.

In this table, β denotes the reading of the barometer, and

M is determined from the formula

$$M = \frac{.0174 \times \text{tabular refraction.}}{\cos \text{ app. altitude.}}$$

TABLE II.

Date of observation.	High Sun.				Low Sun.			
	Sun's hour-angle.	Sun's zenith distance.	Barometer. (β_i).	Air-mass. ($M_i\beta_i$).	Sun's hour-angle.	Sun's zenith distance.	Barometer. (β_i).	Air-mass. ($M_i\beta_i$).
	h m	° ' "	d m		h m	° ' "	d m	
Jan. 28, 1881	0 00	58 29	7.45	14.25	2 57	71 28	7.45	23.24
Feb. 2.....	0 09	57 09	7.39	13.63	3 00	70 45	7.39	22.24
Feb. 17	0 38	52 57	7.43	12.33	2 56	66 09	7.42	18.25
April 22.....	0 12	28 13	7.36	8.35	4 36	66 22	7.36	18.32
April 23, A.M.	0 11	27 49	7.40	8.37	2 45	45 30	7.40	10.56
April 23, P.M.	0 11	27 49	7.40	8.37	4 26	63 57	7.40	16.85
April 29, A.M.	0 06	25 50	7.35	8.17	3 11	48 46	7.35	11.15
April 29, P.M.	0 06	25 50	7.35	8.17	5 23	73 36	7.35	35.73
April 30.....	0 04	25 31	7.41	8.21	3 54	56 31	7.41	13.43
May 28	0 11	19 03	7.32	7.75	5 33	71 14	7.32	22.33

By combining the high- and low-sun observations of each day separately, coefficients of atmospheric transmission are obtained by using the formula

$$\log t = \frac{\log d_{ii} - \log d_i}{M_{ii}\beta_{ii} - M_i\beta_i},$$

where t is the coefficient of vertical transmission by air at a barometric pressure of one decimetre. A tabular statement of these coefficients has been prepared; but the average or adopted value only is here given.

TABLE III.

$\lambda =$.375	.400	.450	.500	.600	.700	.800	.900	1.000
Adopted $t \dots$.884	.892	.909	.923	.942	.955	.965	.970	.971
$t^{7.6} \dots\dots\dots$.392	.420	.485	.544	.636	.705	.763	.794	.799

It is important to notice that (contrary to a generally received opinion) *the transmissibility of the atmosphere is here found to be greatest for the infra-red rays.*

All good noon observations have been reduced to a uniform battery-current of 0.25 weber, and the results arranged in two sets—the first for winter and the second for spring measures.

These tables are not here given ; but the average results are as follows :—

TABLE IV.

$\lambda =$	·375	·400	·450	·500	·600	·700	·800	·900	1·000
Winter d , (mean of 7 series)	·31	88	190	294	328	259	172	111	91
Spring d , (mean of 9 series)	18	57	139	218	281	274	188	121	94

The average noon deflections for winter and spring, given in the previous table, require further correction:—first, for the overlapping portion of the (weak) second spectrum, which is considered from provisional experiments to have here an intensity one thirtieth that of the first; secondly, for the selective absorption by silver surfaces; thirdly, for the selective absorption by one surface of speculum-metal; fourthly, for the diminution of heat in the diffraction-spectrum with increase of the angle of diffraction, which is here provisionally taken as proportional to secant r . The selective absorption by the material of the bolometer is here treated as negligible.

These corrections are expressed as factors by which the uncorrected deflections are to be successively multiplied, except in the case of the first correction, which is to be subtracted. (The second and third corrections have been determined here by special researches on metallic absorption, which will form the subject of a separate memoir.)

The researches here on the selective absorption of lampblack, it should be added, are incomplete, and the value given may be yet subject to a further correction due to this error.

TABLE V.

$\lambda =$	·375	·400	·450	·500	·600	·700	·800	·900	1·000
Correction									
I. (subtr.)	0	0	0	0	0	0	$\frac{1}{30} \times d \cdot 40$	$\frac{1}{30} \times d \cdot 45$	$\frac{1}{30} \times d \cdot 50$
II. (factor)	3·005	2·067	1·606	1·448	1·301	1·227	1·192	1·166	1·145
III. „	2·000	1·923	1·802	1·695	1·550	1·460	1·408	1·389	1·370
IV. „	1·034	1·039	1·051	1·064	1·096	1·138	1·193	1·266	1·366

We have been measuring thus far “heat,” by which we mean the solar energy as interpreted by certain agents (that is, silver, lampblack, &c.) in our apparatus. In the degree in which we have above eliminated the selective absorption peculiar to each of these agents are we entitled to speak of the resultant values as proportional to the solar energy itself. We do not suppose ourselves to have accomplished so untried and difficult a task with exactness, but regard these curves as useful as a first approximation to the absolute-energy curve.

These corrections being applied, the final values of noon deflections at Allegheny become:—

TABLE VI.*

$\lambda =$	·375	·400	·450	·500	·600	·700	·800	·900	1·000
d , (cor.) winter, 1881.	192·6	363·4	579·3	767·9	724·9	527·9	338·3	215·4	173·6
$d_{//}$, „ spring, 1881.	111·9	235·4	423·7	569·6	621·0	552·5	372·3	238·0	234·6

The mean air-mass for winter = 13·88

„ „ „ spring = 9·33

We now proceed to the calculation of the energy outside the atmosphere for homogeneous rays with the data which have been given. For this purpose we have used the formula

$$\log E = \log d - M\beta, \log t,$$

where E is the energy in any ray outside the atmosphere (*i. e.* before telluric absorption), d , the average galvanometer-deflection at noon for the same ray, β , the barometer-pressure in units of one decimetre, or the mass of air in the vertical column, $M\beta$, the corresponding air-mass for the sun's zenith-distance at noon, and t the adopted coefficient of transmission.

The following table has been prepared with the values observed in the spring of 1881, using mean coefficients of transmission, to show the relation between energy outside the atmosphere and that for high and low sun at Allegheny, the various actual absorbing air-masses at the low-sun observations being reduced to a uniform value, double that at high sun.

TABLE VII.

$\lambda =$	·375	·400	·450	·500	·600	·700	·800	·900	1·000
E = energy before absorption	353	683	1031	1203	1083	849	519	316	309
d , = energy after absorption } (corrected high sun)	112	235	424	570	621	553	372	238	235
$d_{//}$ = energy after absorption } (corrected low sun)	27	63	140	225	311	324	246	167	167

E can be computed from d , and $d_{//}$ by the formulæ already given; and with these values the curves in fig. 2 have been plotted.

* It will be seen that, although the winter absorbing air-mass was nearly half as large again as in the spring, the heat received from the shorter wave-lengths was actually greater in the winter. It appears probable, then, that the transmissibility of the atmosphere for the light-producing radiations is relatively greater in winter than in spring. As this effect may be connected in some way with the unequal prevalence of atmospheric moisture at the two seasons, it may be well to state that the tension of aqueous vapour during the winter observations was in the neighbourhood of 2 millimetres, in the spring of 8 millimetres.

The middle curve (I) is that at high sun. Except for the heat below wave-length ($1^{\mu}0$), the area of the curve may be considered to represent the heat actually observed by the actinometers, at noon, as presently given.

The lower curve (II) is that at low sun. Its area is proportional to the heat received when the sun shone through double the absorbing air-mass that it did at noon.

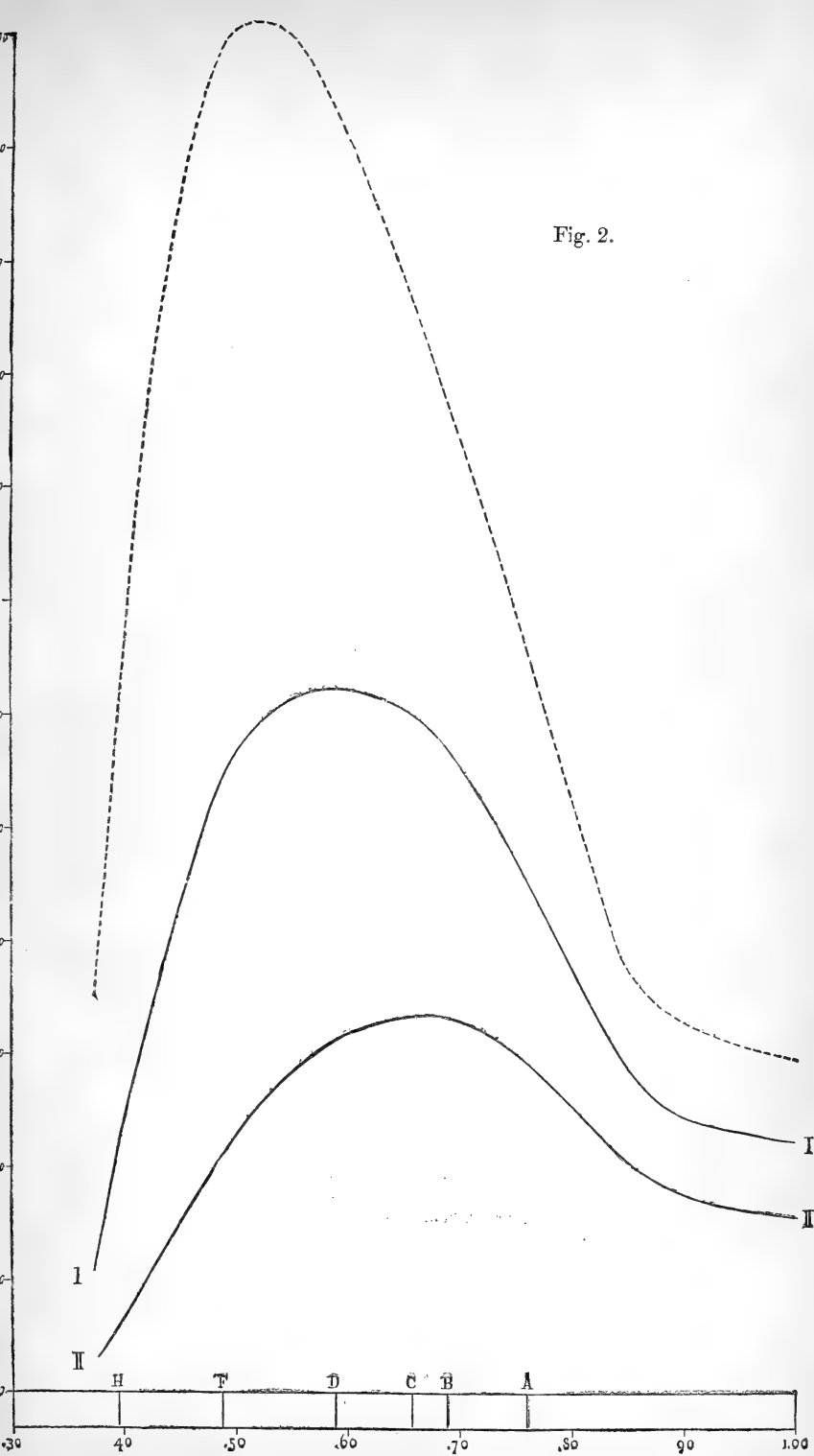
The upper dotted curve is "the curve outside the atmosphere." Its area will give the heat which would be observed if our apparatus were taken wholly above the absorbing air, and the distribution of this heat (energy) before absorption. Knowing the values in calories corresponding to the middle curve, we readily obtain the absolute heat before absorption (the solar constant).

It should be noticed that if we had attempted to deduce this latter value by applying our logarithmic formulæ directly to ordinary actinometric observations (*i. e.* to observations where only the indiscriminate effect of all heat-rays is noted by the thermometer) made at high and low sun, we should have obtained a quite different result. This has been the usual process, but it can never be a correct one; for these exponential formulæ are in theory only applicable to homogeneous rays, and the departure from theory here involves an error which is demonstrably large.

The above values in Table VII. are relative only. To obtain absolute ones we have now to combine this result with the actual measurements of solar radiation in calories, or other units furnished by actinometers under approximately the same conditions. We shall at the same time thus obtain a preliminary value for the solar constant. Taking the mean of our observations with the Violle and Crova actinometers on clearest days, we have 1.81 calory observed at Allegheny in March 1881. This is the absolute amount of heat represented by the area of a completed "high-sun" curve.

To this result the energy distributed through the whole spectrum has contributed, while our bolometer-measurements in the diffraction-spectrum end at wave-length $1^{\mu}00$. Nevertheless, since we do in fact know from subsequent measures (to be given later) where the effective spectrum ends, we can by the aid of these measures prolong the curves and obtain their relative areas with close approximation. In this way we determine, by measuring the charted areas, and making allowance for the (here) uncharted area below $\lambda = 1^{\mu}0$:—

Fig. 2.



Area outside curve above = $1^{\mu}\cdot000$. . .	47.26
Area outside curve below = $1^{\mu}\cdot000$. . .	26.40
Total	<u>73.66</u>
Area high-sun curve above = $1^{\mu}\cdot000$. . .	26.96
Area high-sun curve below = $1^{\mu}\cdot000$. . .	20.00
Total	<u>46.96</u>

The ratio of these areas is $\frac{\text{area outside curve}}{\text{area high-sun curve}} = \frac{73.66}{46.96} = 1.569$.

We have, then, adopting 1.81 cal. as the solar radiation at Allegheny with clear sky, $1.81 \text{ cal.} \times 1.57 = 2.84$ calories as an approximate value of the solar constant.

In all these observations the object has been to avoid the registering of small variations analogous to the Fraunhofer lines, and to give only the general distribution of the energy. The mapping of the interruptions of the energy caused by visible or invisible lines or bands forms a distinct research; and the results are given later in the present article.

We find from these preliminary observations that the maximum energy in the normal spectrum of a high sun at the earth's surface is near the yellow, and that the position of the maximum of heat does not in fact differ widely from that of the maximum of light. It has been long known that certain ultra-violet and violet rays were much absorbed; but it has been supposed that the absorption increased also in the infra-red, so that the luminous part of the spectrum was on the whole the most transmissible.

But we see here, not only how enormous the absorption at the violet end really is, but that the light-rays have suffered a larger absorption before they reach us than the "heat"-rays (*i. e.* than the extreme red and infra-red rays)—a conclusion opposed to the present ordinary opinion, and, if true, of far-reaching importance. For if this "dark" heat escapes by radiation through our atmosphere more easily than the luminous heat enters, our view of the heat-storing action of this atmosphere and of the conditions of life on our planet must be changed. Within the limits of the present charts the "dark" heat apparently does so escape.

We can, from the data now gathered as to the rate of absorption for each ray, compute the value of the heat or energy before absorption (the solar constant) by a new process which is in strict accordance with theory. This preliminary value

indicates that the true solar constant is larger than that commonly given. The ratio of the dark to luminous heat has been so wholly changed by selective absorption that we must greatly modify our usual estimates, not only of the sun's heat-radiation, but of his effective temperature.

The sun to an eye without our atmosphere would appear of a bluish tint.

In spite of the care with which the experiments on which the above conclusions rest have been conducted, owing to the importance of the subject, and to their departure in some respects from received opinion, it seemed desirable to repeat them under conditions differing as much as possible from those in which they were made. If the preceding conclusions are sound, we ought to find like results by actually ascending to a great height and directly measuring there, as well as below, the absorptions which each ray has actually undergone.

Expedition to Mount Whitney.

In July 1881 an expedition, fitted out and instrumentally equipped at the Allegheny Observatory, proceeded in the writer's charge, but with the aid of transportation furnished by the War Department and under the official direction of General W. B. Hazen, U. S. A., Chief Signal Officer of the Army, to Mount Whitney in Southern California, where these observations and others were repeated at two contiguous stations at very different absolute altitudes. The results will shortly appear in an official publication (some of the drawings prepared for which have been used for the present memoir by the kind permission of General Hazen). At present it is sufficient to say of them, that the conclusions just rehearsed were confirmed and extended.

While on the mountain, at an elevation of 13,000 feet, a hitherto unrecognized extension was observed by the bolometer of the infra-red *prismatic* spectrum in the vicinity of the great absorption-band marked on our prismatic chart as Ω , and beyond it; and on the return to Allegheny it was found that this last observation could still be continued in our lower atmosphere. The generosity of a citizen of Pittsburg had enabled the observatory to provide for the expedition several pieces of special apparatus. Among these was a Foucault siderostat, of the dimensions of that at the Paris Observatory, but of a much improved design, and a special apparatus (the spectro-bolometer) to enable the deviations of the invisible rays to be measured with an error of less than a minute of arc &c. ; and this apparatus has been used in the new research we now describe. (The siderostat was made by A. Hilger, of

London, the spectro-bolometer by W. Grunow, of New York ; and both have been very satisfactory.)

Sir William Herschel, in 1800*, showed that heat extended below the visible spectrum. He found that about one half the spectrum consisted of obscure, and one half of luminous heat. Seebeck and Melloni in various memoirs showed that the disposition of the heat depended on the substance of the prism, and that this was due in part to its absorption.

In 1840 Sir John Herschel† gave a thermograph of the invisible spectrum, indicating unequal absorption below the red.

Dr. J. W. Draper, in 1842‡, observed three wide bands in this region, which he called α , β , γ . In 1846 MM. Foucault and Fizeau appear to have observed the same lines. Dr. Draper§ states that prior researches lead him to believe that the hottest part of the normal spectrum will be found in the yellow.

Dr. J. Müller|| gives a construction showing how we may, from the distorted prismatic spectrum, obtain the true or normal dispersion. Dr. Müller conjectures that the wave-length of the extremest infra-red ray is about $1^{\mu}8$; and from his diagram it appears that nearly two thirds of the heat is below the visible portion.

Tyndall¶ gives the position of the maximum of heat in the prismatic spectrum, and estimates the invisible radiation of the sun to be twice the visible.

In 1871 Lamansky** gave a drawing showing three gaps in the continuity of the infra-red curve as observed by the thermopile. Lamansky repeats the usual statement that these infra-red rays are strongly absorbed by the atmosphere.

In 1879 M. Mouton††, in a valuable memoir, speaks of there being four known bands in the infra-red whose wave-lengths are $0^{\mu}85$, $0^{\mu}99$, $1^{\mu}23$, $1^{\mu}48$, and gives $1^{\mu}77$ and $2^{\mu}14$ as wave-lengths he supposes himself to have identified.

If our charts be correct, there is no considerable band at $1^{\mu}48$; and $2^{\mu}14$, which he marks as the termination of the spectrum, is in fact the hottest point in its neighbourhood.

It seems probable, however, that he had perceived by his ingenious method the existence of the band whose wave-length

* Phil. Trans. 1800.

† Ibid. 1840.

‡ Phil. Mag. May 1843.

§ Ibid. 1847.

|| Poggenorff's *Annalen*, vol. cv.

¶ Phil. Trans. 1866.

** *Monatsbericht Königl. Akad. Wissenschaft. Berlin*, 1871; *Phil. Mag.* 1872, vol. xliii.

†† *Comptes Rendus*, vol. lxxxix. p. 291, and vol. lxxxiii. p. 1190.

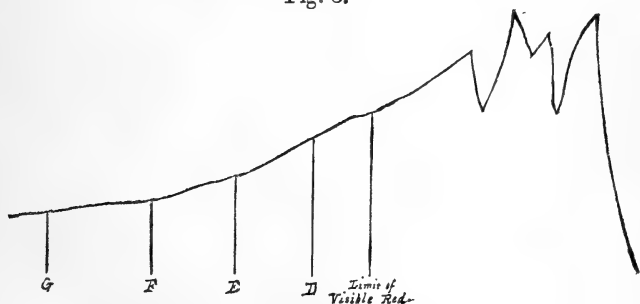
on our charts is marked $1^{\mu}37$, and in doing so had reached the furthest band then certainly observed.

Captain Abney*, in 1880, mapped by photography the infra-red prismatic spectrum as far as wave-length $1^{\mu}075$ with a precision and completeness till then wholly unknown, besides giving the wave-lengths of lines for which he derives by an extrapolation curve a position at $\lambda = 1^{\mu}240$, and indicating a band still beyond.

Captain Abney had previously published a map of the diffraction-spectrum extending to $\lambda = 0^{\mu}9682$. Dr. J. W. Draper†, by the aid of Captain Abney's map, believes he has identified the lines α , β , γ he saw in 1842 with groups represented by Abney at $\lambda = 0^{\mu}8150$ to $0^{\mu}8350$, $0^{\mu}8930$ to $0^{\mu}9300$, and $0^{\mu}9350$ to $0^{\mu}9800$.

On our chart we have given Draper's α , β , γ according to his own locations of them. He believes these to be the same lines seen by himself, Foucault and Fizeau, and Lamansky. According to Draper, then, the lowest limit of his own or any other researches known to him in 1881 did not extend much beyond wave-length $1^{\mu}0000$. It appears to us probable, however, that Lamansky's lowest point was below this; and we give a copy of Lamansky's curve (fig. 3), which the reader can compare with the positions on our present charts.

Fig. 3.



LAMANSKY.

These brief references concern only what belongs to our immediate purpose, and are not offered as a history of the subject.

Recent Observations on the Invisible Prismatic Spectrum.

After the return from Mount Whitney, observations were taken at Allegheny with the train of apparatus used on the

* Phil. Trans. 1880.

† Proceedings of the American Academy, 1881.

mountain just referred to, and on nearly every good day during the first six months of 1882. There were of such:—4 days in January, 8 in February, 9 in March, 9 in April, 9 in May, 12 in June; in all, 51 days.

Very early the observations with this efficient apparatus (the result of improvements due to the previous two years' practice) showed, by an accuracy not hitherto attained in such measures, the possibility of mapping the regions of the infra-red spectrum believed to have been first observed on Mount Whitney, and which have remained hitherto unknown. The extreme narrowness of the bolometer-thread (one fifth of a millimetre) and the size of the prismatic spectrum employed made it also possible, in spite of the condensation of the latter, to discriminate lines and gaps in its continuity which had escaped previous observation*. The spectrum, as the reader now sees it in the charts, was mapped with the intention of noting these interruptions of energy, which had been in the previous research designedly neglected. The bolometer shows the cold in the principal visible Fraunhofer lines readily; but as their effect individually is slight, it has not been indicated in the part of the visible spectrum above C.

The map reached approximate completeness early in April 1882, after which date observations have been still continued daily, whenever possible; so that every portion of the curves here given, to the smallest inflection, has been observed from three to twenty times, and the accidental variations due to momentary interruptions of solar heat by invisible clouds have been, it is hoped, nearly eliminated.

The bolometric work, represented by the preceding 51 days' observations, may be here regarded as being divided into two classes having distinct though related objects.

1st. To determine the general *selective* absorption of the earth's atmosphere throughout the entire spectrum in connexion with the observations already made here and on Mount Whitney. For this purpose measurements have been made at the following deviations:— $44^{\circ} 30'$, $45^{\circ} 30'$, $45^{\circ} 53'$, $46^{\circ} 12'$, $46^{\circ} 30'$, $46^{\circ} 45'$, $47^{\circ} 30'$, $48^{\circ} 00'$, $49^{\circ} 00'$, $50^{\circ} 00'$, $51^{\circ} 00'$.

All these points have been measured on, twice each, the whole forming a "series;" and these "series" are observed at least twice daily—namely at meridian, and when the air-mass

* Nevertheless, as the thread, however narrow, is not absolutely linear, it feels the cold before its centre coincides with the centre of the line. The interruptions of the energy-curve are thus in fact all in a slight degree too wide, especially at the commencement of each depression; and it is probable that the bands we have marked are really due to an aggregation of finer lines.

is approximately double that at meridian ; or else three times daily—at meridian, when the mass of air traversed is approximately $1\frac{1}{2}$ time that at meridian, and again when it is approximately $1\frac{1}{2} \times 1\frac{1}{2} = 2\frac{1}{4}$ times that at meridian.

It will be observed by a reference to the map, that the points chosen for measurement coincide, as a rule, with the summits of the energy-curve; but separate investigations are still in progress on the nature of the absorption in the intervals, to determine whether the newly observed bands are of solar or terrestrial origin. The part of the spectrum included extends from $\lambda = 0^{\mu} \cdot 383$ (above H in the violet) to $\lambda = 2^{\mu} \cdot 28$ (in the newly observed infra-red region, about two octaves below Fraunhofer's B).

(As we can, in fact, obtain evidence of heat in ultra-violet waves whose length is little more than $0^{\mu} \cdot 3$, the length of the solar spectrum as now observable by the bolometer is between 3 and 4 octaves.)

The distant slit is separately exposed at each observation, and the extremity of the full swing of the galvanometer-needle is read. In all these measures the galvanometer is used in the same condition of sensitiveness. The slit is opened to a constant width of 2 millim.* (except in measuring the very feeble energy at the most-refrangible end of the spectrum, where the width has been increased without prejudice to accuracy, owing to the corresponding prismatic expansion of the spectrum itself). The same bolometer is used, as a rule having for this purpose 1 millim. effective aperture (except in measurements at the most-refrangible end of the spectrum, where the full aperture of the bolometer is used).

These observations on the absorption of different air-masses for each spectral ray evidently furnish means for determining the curve outside the atmosphere, by the method already indicated. They also, of course, give us the means of making a map of the whole spectrum ; but their use for this latter purpose is incidental.

2nd. The other class of observations is for the special purpose of making a spectral map, extending from the line C to the lower limit of the infra-red.

This is carried on by means of the linear bolometer, consisting of a single strip $\frac{1}{2}$ millim. wide. In this second class of observations, a rough map of the whole infra-red spectrum having been prepared, a very limited part of the spectrum

* It will be remembered that, the actual distance of the slit being 5 metres, this aperture subtends an angle little greater than one minute of arc.

(such as that included between $15'$ of deviation) is gone over several times in the course of one day, the measurement being repeated on every single minute of arc, with a separate opening and closing of the slit, and a record made of the full swing of the galvanometer-needle for each observation.

These observations are entered numerically, and corresponding charts made on large sheets of section-paper. The same narrow region will thus be gone over also on different days, and the different charts subjected to a very rigid examination; so that every feature which is not common to them all is rejected or reexamined; and in this manner the whole spectrum is studied. These original charts are on a scale four times as large linearly as that the reader now sees (Plate III.).

In addition to this, on some clear days, tracings have been made upon the chart directly corresponding to the movements of the galvanometer-needle: that is to say, the observer at the spectro-bolometer has moved the bolometer through the whole spectrum by means of the tangent-screw; the slit has been left permanently open, so that the bolometer has been constantly exposed; and the observer at the galvanometer, seeing the needle moving, as a hot or cold part of the spectrum passes over, calls the deflection of the galvanometer corresponding to each minute of arc, while a third person plots the same on section-paper. In this way as many as eight curves, like those here given of the prismatic spectrum, have been obtained between noon and sunset on one day, giving a picture of the action of the selective absorption of the atmosphere in every part of the spectrum, as the rays of the sinking sun pass through greater depths of air. This third method (very useful when, as in this case, many observations have to be taken in a short time) is nevertheless less accurate than those before described.

A careful bolometric and also optical setting is made on some well-known line, usually C, at least once daily, to make sure that the adjustment of the instrument is equally accurate for the visible and invisible rays.

Description of the Apparatus.

The rays of the sun are reflected horizontally from the mirror of the large Foucault siderostat through an aperture in the north wall of the observatory, and received upon a plate with a slit, whose jaws, moving each way from the centre by a micrometer-screw, can be regulated so as to allow a beam of any desired size to pass. $4\frac{1}{2}$ metres from this slit, at the distance of its principal focus, is a collimating lens, L (fig. 4, p. 174), of a

special kind of flint glass which has been found nearly transparent to all the invisible rays measured. This lens and the slit are fitted into opposite ends of a tube, T, $4\frac{1}{2}$ metres long, held by suitable y's. The beam of rays from the slit, now rendered parallel by the collimator, next falls upon a prism*, P, of the same kind of glass as the lens, supported on a circular adjustable table over the vertical axis of the massive instrument we have called provisionally the spectro-bolometer.

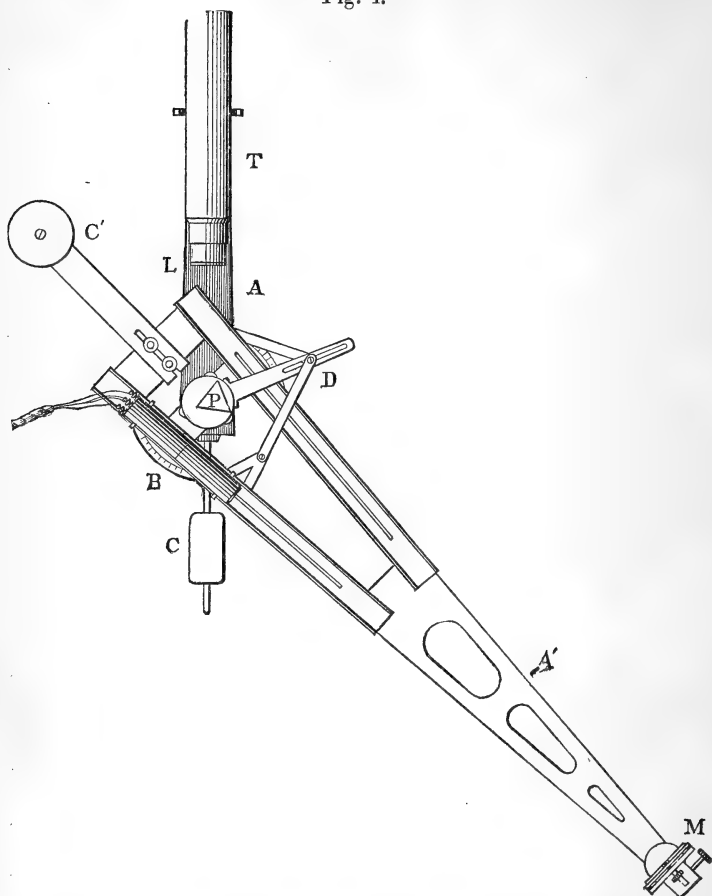
Whatever the sensibility of the apparatus to heat, it is evident that we cannot accurately map the narrow spectral limits between contiguous heat and cold, unless we can fix their position with exactness. Especially when we consider that these rays are invisible, and that the whole process may be compared to a patient groping in the dark, does the need of an instrument which will record the precise point where a hot or cold line was felt become obvious. This is the object of the spectro-bolometer, which, as well as other apparatus mentioned here, will be described more particularly in the account of the observations made on Mount Whitney. (It was made by W. Grunow, of New York, from the writer's design.)

Two long arms, A, A', turn independently about the above-mentioned axis, the angle between them being measured by a graduated circle with two verniers reading to $10''$. One of these arms is directed toward the slit, and the other toward the spectrum formed by the light on leaving the prism. This latter arm carries at its extremity a concave mirror, M, of 98 centim. focus, and on either side of the prism an accurately planed track directed toward the centre of the mirror, on either of which slides a carriage with y's. Into these y's, at B, drops either of two "ebonite" cylinders, one containing the bolometer and the other the ordinary reticule and eyepiece. The bolometer used in the measurements for these maps exposes to the spectrum a single vertical strip of platinum, $\frac{1}{5}$ millim. wide, covered with lampblack, and placed accurately in the axis of the ebonite cylinder by reversal under a compound microscope. The eyepiece also has its cross wires centred in the second cylinder, and serves to examine optically the place which will be occupied by the bolometer-strips when the bolometer-cylinder is in the y's. The optical axis

* This prism, whose optical properties are in every way excellent, was made by Mr. A. Hilger, of London. Its principal constants are as follows:—size of polished faces, 53 by 49 millim.; specific gravity, 2.895; refracting angle, $62^{\circ} 34' 43''$; index of refraction for D line, 1.5798; index of refraction for H line, 1.6070. (A rock-salt prism of nearly equal size and great purity, as well as prisms of quartz and spar, have been used to determine the absorption of the glass for each ray, visible and invisible.)

of the mirror M exactly bisects the angle between the direction of the arm A' and the central line of the track; so that

Fig. 4.



a ray falling on the centre of the mirror from the centre of the instrument at P, after reflection falls upon the bolometer-strips. C, C' are counterpoises to offset the weight of the arms A, A'.

To adjust the apparatus for observation, the screws at D are loosened, the prism removed, and the arm A' brought around in line with the long tube. The eyepiece being placed in the y's at B, the image of the distant slit is brought upon the central wire, when the reading of the divided circle should be $0^{\circ} 00' 00''$, indicating a deviation of zero. The arm is then moved to one side as in the figure, until the mirror intercepts

the rays from the prism, which has first been replaced upon its table and adjusted by the screws below. The prism is now carefully set to minimum deviation (usually for the D_1 line), and is then automatically kept in minimum deviation for all other rays by the tailpiece and attachment at D. When the cross wires of the eyepiece are set upon the D line, the circle should indicate a deviation of $47^\circ 41' 15''$. A bright and pure image of the spectrum, about 6 millim. wide and 640 millim. long, between the A and H lines is now formed in the principal focus of M near the prism; and the bolometer-case being substituted for the eyepiece, the carriage is slid along the track until the central strip, placed vertically and parallel to the Fraunhofer lines, comes exactly into focus. The heat of the solar rays in any part of the spectrum may now be measured by the bolometer (the galvanometer giving a marked deflection as it passes over the leading Fraunhofer lines); and the deviation for that part is exactly indicated by the divided circle.

The galvanometer used in connexion with the bolometer is a Thomson reflecting astatic galvanometer of about 20 ohms resistance, constructed especially for the purpose by Elliot Brothers, of London. It is placed upon a pier entirely disconnected from the building. The scale is cylindrical, with divisions 1 millim. apart on a transparent surface, and is placed 1 metre from the galvanometer mirror. Since the whole deflection ordinarily employed does not exceed 5° , as a rule the reading of the galvanometer requires no reduction for our present purpose. A resistance-box forming the Wheatstone's bridge and other electrical adjuncts of the bolometer are on the right of the galvanometer-pier. The rheostat is in a convenient position near the scale; and the battery-galvanometer, for measuring and regulating the strength of the current used, is on a pier in another part of the room.

In conducting the measurements for mapping the spectrum, one observer is usually at the spectro-bolometer to set the circle to the deviation required, to see that the light from the siderostat falls properly upon the prism, and to admit the sunlight at a given signal by means of cords attached to a sliding cover in front of the slit where the sun's rays first enter the room. Another observer, placed at the galvanometer, reads the corresponding indications of the instrument; and a third enters them in form in a book prepared for the purpose, and gives the signal for exposure. As all these observations are carried on in a partially darkened room, a fourth person is usually stationed without to wind the siderostat clock, and to give notice of any passing clouds to those within the building.

Example of the Mode of Observation.

As an example of the first class of measures, let us consider the observations made with the Hilger prism on June 22, 1882. The high-sun observation was made at 0^h 15^m. The sun's zenith-distance at this time was 17° 10'; the air-mass* was 1·047 time the mass overhead; the height of the barometer corresponding to the air-mass overhead was 7·39 decimetres; consequently the air-mass for a zenith-distance of 17° 10' was $7·39 \times 1·047 = 7·74$ decimetres.

The sun's zenith-distance at 6^h 25^m (the time of the second observation) was 79° 8' †; the height of the barometer was the same as at noon; and the air-mass by the same formula was 5·18 times that overhead, or $7·39 \times 5·18 = 38·27$ decimetres; so that the mass of air traversed in the second observation exceeded that in the first by an amount capable of supporting 30·53 decimetres of mercury.

The galvanometer-deflection obtained in the part of the spectrum whose deviation is 44° 30' (a part which is near the extreme lower limit of the present observations, far below the visible red) was at noon 17, and in the afternoon 11. In the violet, where the deviation is 50° 00', the corresponding deflections were 4·5 and 0·39. Let us take these two feeble extreme rays as types with which to illustrate our process. Considering first the infra-red ray, we have deflection at noon $= d_1 = 17$, deflection in afternoon $= d_2 = 11$, difference in mass of air traversed $= M_2\beta_2 - M_1\beta_1 = 30·53$ decimetres, which by its absorption has produced the difference in the deflections. t representing the amount of energy transmitted by a layer of air equivalent to 1 decimetre of mercury, we find from the formula

$$t = (M_2\beta_2 - M_1\beta_1) \sqrt{\frac{d_2}{d_1}}$$

$t = .986$; that is, a mass of air capable of supporting 1 decimetre of mercury in the barometer transmits 98·6 per cent. of the energy of this particular kind of ray. This quantity t we call the coefficient of transmission of the ray.

Knowing now the amount of energy transmitted by one such layer of air, we can find the amount transmitted by the 7·74 layers which intervened between the observer and the sun at noon, namely $.986^{7·74} = .895$. Only 89·5 per cent.,

* Computed from the formula $M = \frac{\text{tabular refraction}}{\cos \text{app. altitude}}$.

† In general it is not advisable to make observations at so great a zenith-distance as this.

therefore, of the original unknown heat of the ray, which we will represent by E , reached the observer at noon, producing a deflection of 17, or $\cdot 895 E = 17$, giving

$$E = \frac{17}{\cdot 895} = 19\cdot 0;$$

that is, had our instrument been placed outside the atmosphere at that time, it would have indicated a deflection of 19 instead of 17.

By a similar process we find that the coefficient of transmission for the violet ray is $\cdot 923$; from which we see that the ultra-red ray is transmitted with greater facility than the violet. The amount of this violet radiation transmitted by the whole depth of atmosphere at noon was $\cdot 538$, from which its energy outside the atmosphere was $\frac{4\cdot 5}{\cdot 538} = 8\cdot 4$.

The table below gives the coefficients of transmission &c. for these and other points in the spectrum where measurements were taken on this day. The first column gives the deviation of the observed ray in the spectrum of the prism used; the second and third columns the deflections obtained with the galvanometer at noon and in the afternoon respectively; the fourth column the coefficient of transmission (for an atmosphere supporting one decimetre of mercury); the fifth the transmission of the whole depth of atmosphere at noon, obtained by raising the coefficient of transmission to the 7.74 power; and the last the computed energy outside the atmosphere expressed in galvanometer-deflections.

TABLE VIII.

Deviation.	d_1 .	d_2 .	t .	$t^{M\beta_1}$.	E .
53 00	0.02	0.00			
52 00	0.21	0.00			
51 00	0.96	0.09	.925	.549	1.8
50 00	4.5	0.39	.923	.538	8.4
49 30	7.3
49 00	13	3.0	.953	.689	18.9
48 00	43	12.5	.960	.731	58.8
47 30	72	38	.979	.850	84.7
46 45	158	109	.988	.910	173.7
46 12	209	134	.986	.894	233.8
45 53	175	107	.984	.883	198.3
45 28	122	79	.986	.895	136.2
44 30	17	11	.986	.895	19.0

Similar reductions have been made for each day's observation, the result from each being confirmatory of the statement here (see column $t^{M\beta_1}$) that the atmospheric absorption *dimi-*

nishes continuously as the wave-length increases (save for the interruptions already cited) to the extremity of our charts.

The graphic representation of this and other extratelluric curves of energy will be given in a later memoir, in such a form as to show from the mean of a year's observations the percentage of absorption suffered by each ray in the entire spectrum, visible and invisible.

The reader who may desire still fuller details as to the apparatus, the original observations, and their treatment, is referred to the forthcoming official publication already mentioned. In the later memoir will be found a description of the method used for determining the wave-lengths corresponding to measured deviations, and the formulæ for deducing from the prismatic spectrum the distribution of the energy and the extent of the spectrum on the normal scale.

Summary.

As one result of this present research, the chart of the prismatic spectrum as observed at Allegheny with the bolometer is now presented (Plate III.). The abscissæ are proportional to deviations, and the ordinates to measured energies. The second chart now given (Plate III.) represents the normal spectrum as deduced from the prismatic; as it has been thought advisable to present it here for the reader's convenience, in advance of a description of the means used for making it. The abscissæ on this are proportional to actually measured wave-lengths, and the ordinates to measured energies. In both charts the area between ordinates corresponding to like wave-lengths is the same; and hence the total areas are the same. Their very dissimilar contour is due to the prismatic distortion.

Faint indications of solar energy below the lowest point here shown have been found; and these, with some considerations as to the nature of the new absorption-bands, may be given hereafter, together with tables (already prepared) of the absorptive action of the *solar* atmosphere for each spectral ray. These will, it is hoped, give with a satisfactory approximation the distribution of the energy before any absorption whatever at the source—that is, of the energy in the photosphere itself.

The extent of the newly observed region may be most clearly seen by reference to the map of the normal or diffraction-spectrum (Plate III.). Previous maps end at or near wave-length $1^{\mu}2$. Beyond this point (with the exception of the single band near wave-length $1^{\mu}4$) every line, and every ordinate representing heat, is believed to be new. The extent of the region here newly mapped is then considerably

larger, on the normal scale, than the whole of that (both visible and invisible) previously known.

We observe that the prismatic spectrum is enormously expanded at the violet end. To carry this on the prismatic scale to wave-length $0^{\mu}3$ would extend it far beyond the limits of our chart. *All* the actual energy in the entire ultra-violet part, however, is insignificant—how insignificant can best be seen by reference to the normal chart, where the minute area beyond wave-length $0^{\mu}4$ represents the *whole* ultra-violet energy.

We are accustomed to speak of the ultra-violet and infra-red regions without reflecting on the enormous difference between their actual importance. The reader will be able to see by a simple inspection of the normal chart and a comparison of the little area above wave-length $0^{\mu}3$, and the great area below wave-length $0^{\mu}7$, that the latter is nearly a hundred times as great as the former. Yet the former, owing to the prismatic expansion, and to the selective absorption of the feeble rays of this region by certain salts of silver, with which it can be photographed (while the far greater energy below makes little impression on these salts), has occupied more attention than the latter. When we observe here how the infra-red region is compressed by the prism, we can understand how its extent has been underestimated. Its real extent is so vast that we should accustom ourselves to consider “in the infra-red region” a wholly vague term, needing to be supplemented with a description of the particular part of the infra-red referred to.

It may be well to epitomize the principal results of all these researches as far as they have been here given. In general they emphasize and extend our first conclusions.

1st. In measures now made for the first time on approximately homogeneous rays in the diffraction-spectrum, we find that the maximum energy is above the red, and is placed in fact near the yellow. The place of this maximum point varies with the sun's altitude, ranging from a wave-length of nearly $0^{\mu}55$ on a clear day and with a high sun, to a wave-length of $0^{\mu}65$, or even more, before sunset. On the normal scale, then, the position of the maximum of heat in the spectrum does not vary widely from that of the maximum of light. It is shown later how similar results are deducible from the prismatic spectrum.

2nd. By comparing the ordinates for high and low sun in different parts of the spectrum we see that they grow unequally, indicating an enormous systematic absorption, increasing toward the ultra-violet and diminishing toward the

infra-red; and these ordinates not only indicate its character, but give its amount. In contradiction to the statement of many investigators and of present opinion on the point, we find that (according to these measures) the absorption grows on the whole less and less as we go down below the red to a point near wave-length $2^{\mu} \cdot 8$. By this it is not meant to deny the existence of regions of very great local absorption in the lower spectrum. These same observations do in fact point out new regions of such local absorption. But, excepting these, they warrant us in saying that, broadly speaking, the absorption through the whole spectrum, visible and invisible, appears to follow one simple law, and to decrease where the wave-length increases; so that not only is the ultra-violet more absorbed than blue, blue than yellow, and yellow than red, but that red is more absorbed than the infra-red, and each degree of infra-red is more so than the next one below it.

3rd. By the use of the ordinary logarithmic formula, here employed in its legitimate application to homogeneous waves, we can pass from the curve inside to that outside the atmosphere; in other words, we can virtually transport our observing-station to a point wholly above the air, and determine the distribution of the sun's heat before this unequal absorbent action of our atmosphere has affected it. We need only embody the results for selective absorption given by our tables in a simple graphic construction (like that here shown in connexion with the preliminary investigation) to see that the point of maximum heat *outside* our atmosphere lies near wave-lengths $0 \cdot 50$ to $0 \cdot 55$ —or, as we are entitled to say, that the hottest portion of the spectrum outside the atmosphere will be found rather in the green than, as here, near the yellow.

It is probable, from our measurements, that the sun would appear of a decidedly bluish tint to the naked eye placed without our air.

This atmosphere, which we are so accustomed to regard as colourless, has then, in fact, played a part analogous to that of a yellowish or reddish glass, whose impure colour is not a monochromatic yellow or red, but a compound of all spectral tints in unaccustomed proportions. Had we in all our lives had no light but from an electric light, seen through such a reddish glass shade, we should probably have believed this reddishness to be the "natural" or proper colour of the naked carbons, and moreover that it represented "the sum of all radiations." It would apparently answer, in an individual brought up in ignorance of any other light, to our common notion of *whiteness*; so that even though it really possessed

colour, the medium would (previous to investigation) be deemed colourless. In the same way common opinion regards our air as colourless; yet it cannot be so, but must necessarily (according to these observations) be considerably coloured.

As we have been accustomed to regard it as colourless, however, it is clear that were it removed we should, in seeing the sun's true appearance for the first time, regard the sun itself as coloured.

Our white light, then, is *not* the sum of all radiations, but only of a part, even of the visible ones.

4th. We can, by measuring the area of the curve outside our atmosphere and comparing it with the area of the curve within, obtain by a method never before pursued, which is in close accord with theory, a value for the solar constant.

Previous observations have found from 1.7 cal., in the time of Pouillet, to 2.5 cal. in that of Violle, with a tendency to increase. The value here given from our preliminary investigation is 2.84 cal. The last figure is of little weight; and the exactness of that in the first decimal place is probably open to doubt. The conclusion which we are entitled to draw from these investigations in the stage here presented is that the solar constant is in reality greater than has been heretofore supposed, and that it is probable that it is not very greatly inferior to 3 calories. This important point will be discussed fully in connexion with the Mount-Whitney observations, with which the complete graphical constructions elucidatory of our present tables will be given.

5th. These observations show heat in extreme ultra-violet rays, and the change of temperature (hitherto unobserved) in the Fraunhofer lines. They lend increased probability to the belief that *all* the energy in any ray can be exhibited as heat, if there be a proper medium to receive this energy. Their evidence, so far as it goes, then, favours the conception of one solar energy, which is interpreted in terms of heat, or of light, or of chemical action, according to the medium by means of which we choose to observe it.

6th. The *ratio* of luminous to dark heat has evidently been wholly changed by the selective absorption. The ratio at the sea-level may be found with close approximation by measuring the two areas—(1) above the point where we assume the luminous spectrum to end, and (2) below it. This point each one may define differently; for the extent of the luminous spectrum depends much upon our precautions for observing it. If we assume it to end near B, then three quarters of the energy must be termed invisible; if at the actual visual extremity (far below A), then less than half. To fix our ideas, let us

suppose it to terminate at Fraunhofer's A. We then find :—

Luminous and ultra-violet energy (within the smooth curve)	} 0.368
Infra-red energy	0.632
	<hr/> 1.000

The ratio of the invisible (infra-red) to the whole, then, is 0.632; and there is reason to believe this value rather too small than too large. If, however, we deduct the space occupied by the gaps in the lower spectrum, the ratio becomes 0.562. The infra-red energy at sea-level may be roughly taken, as thus defined, at three fifths of the whole. At the same time the ratio of luminous to obscure energy without our atmosphere is, we repeat, far greater than within it.

We conclude (among other consequences of our observations) that since the heat in the shorter wave-lengths (corresponding in a general sense to high solar temperature) was thus relatively greater before absorption, that we are obliged to increase our usual estimates, not only of the amount of heat the sun sends us, but (and very greatly) of the effective *temperature* of the solar surface.

The relatively small amount of energy corresponding to great wave-lengths in the infra-red is due not so much to absorption as to the fact that there is no considerable solar energy existing there at all. The relatively great amount of energy in the luminous part of the spectrum exists there, not on account of a feeble absorption, but *in spite* of a strong absorption; and the original solar energy here was even much more considerable than what we see.

It is probable, however, that the solar spectrum before absorption, though originally weak below the red, *yet extended very much further into the infra-red than our charts indicate*. We may even regard it as probable that some agent of the atmosphere acts as an almost complete barrier to the entrance or departure of rays below the point charted.

It should be understood that these researches have here a practical bearing of great consequence. The temperature of this planet and with it the existence, not only of the human race but of all organized life on the globe, appears, in the light of the conclusions reached by the Mount Whitney expedition, to depend far less on the direct solar heat than on the hitherto too little regarded quality of *selective* absorption in our atmosphere which we are now studying.

The discussion of these and other points is reserved for a subsequent memoir. Among these will be the fuller consideration of the place of the principal absorption of water

vapour, a consideration which it will be advantageous to present in another connexion. It is to be remembered that all the values here given are presented as approximate, and not as final ones.

In presenting these researches, on the part of the Allegheny Observatory, I should state that the considerable especial expenditures they have involved have been met by the generosity of a friend of that institution, whose aid, which alone made them possible, I would gratefully acknowledge.

In conclusion I desire to say that I have been aided throughout them by Messrs. F. W. Very and J. W. Keeler of this Observatory, with an efficiency and interest in their prosecution without which they could hardly have taken their present form.

Allegheny Observatory, Allegheny,
Pennsylvania, December 30, 1882.

XXIII. *On the Spectra formed by Curved Diffraction-gratings.*
By WALTER BAILY*.

[Plate II.]

IN the curved diffraction-gratings invented by Professor Rowland, he has pointed out that if a source of light be placed at the centre of curvature, all the rays diffracted back from the grating will have their foci on the circle which lies in a plane perpendicular to the lines of the grating, and is described on the radius of curvature as diameter. In this paper I investigate the locus of these foci, and of those of rays transmitted through a transparent grating for any position of the source of light in the same plane.

Let a plane grating be placed at D (fig. 1) with its lines perpendicular to the paper and one of them passing through D, and its plane perpendicular to CD, and let aD be an incident ray, of which a portion with wave-length λ_1 is diffracted along Da_1 , and a portion with wave-length λ_2 is diffracted along Da_2 , a_1Da_2 being a straight line. Let $CDa = \theta'$, and $CDa_1 = \theta$; and let σ be the distance between the lines of the grating. Let D' be the next line of the grating to D; and draw $D'a'$ and $a'_1D'a'_2$ parallel to Da and a_1Da_2 respectively; and draw aa' , $a_1a'_1$, $a_2a'_2$ perpendicular to Da and a_1Da_2 . Then we must have

$$aD + Da_1 = a'D' + D'a'_1 + n_1\lambda_1$$

and

$$aD + Da_2 = a'D' + D'a'_2 + n_2\lambda_2,$$

where n_1 and n_2 are integers.

* Read before the Physical Society of London, January 27, 1883.

These equations give us

$$\sigma (\sin \theta' + \sin \theta) = n_1 \lambda_1,$$

$$\sigma (\sin \theta' - \sin \theta) = n_2 \lambda_2.$$

Now (fig. 2) let D be the centre of a cylindrical grating whose lines are perpendicular to the plane of the paper, C the centre of curvature, and $CD=c$. Let P be the source of light, Q the focus of a diffracted ray, E a point on the grating near to D. Join PD, PE, QD, QE, CE. Let $CP=a$, $CQ=b$, $\angle PCD=\alpha$, $\angle QCD=\beta$; $DP=r$, $DQ=r'$, $\angle CDP=\theta$, $\angle CDQ=\theta'$, $\angle DCE=\gamma$. Then we have for light diffracted back from the grating, which we may call "reflected light,"

$$\sin CEP + \sin CEQ = n \frac{\lambda}{\sigma},$$

$$\frac{a \sin (\alpha - \gamma)}{\{a^2 + c^2 - 2ac \cos (\alpha - \gamma)\}^{\frac{1}{2}}} + \frac{b \sin (\beta - \gamma)}{\{b^2 + c^2 - 2bc \cos (\beta - \gamma)\}^{\frac{1}{2}}} = \frac{n\lambda}{\sigma}.$$

Expanding in terms of γ , we get

$$\begin{aligned} & \frac{a \sin \alpha}{\{a^2 + c^2 - 2ac \cos \alpha\}^{\frac{1}{2}}} + \frac{b \sin \beta}{\{b^2 + c^2 - 2bc \cos \beta\}^{\frac{1}{2}}} - \frac{n\lambda}{\sigma} \\ & + \left[\frac{(a^2 + c^2 - 2ac \cos \alpha) a \cos \alpha - a^2 \sin^2 \alpha}{(a^2 + c^2 - 2ac \cos \alpha)^{\frac{3}{2}}} \right. \\ & \quad \left. + \frac{(b^2 + c^2 - 2bc \cos \beta) \cos \beta - b^2 \sin^2 \beta}{(b^2 + c^2 - 2bc \cos \beta)^{\frac{3}{2}}} \right] \gamma \\ & + \text{terms involving higher powers of } \gamma = 0. \end{aligned}$$

Putting

$$a \cos \alpha = c - r \cos \theta, \quad b \cos \beta = c - r' \cos \theta',$$

$$a \sin \alpha = c \sin \theta, \quad b \sin \beta = c \sin \theta',$$

we get

$$\sin \theta + \sin \theta' - n \frac{\lambda}{\sigma} + \left[\frac{\cos \theta}{c} - \frac{\cos^2 \theta}{r} + \frac{\cos \theta'}{c} - \frac{\cos^2 \theta'}{r'} \right] c \gamma + \&c. = 0.$$

This equation must be satisfied for all very small values of γ .

Hence

$$\sin \theta + \sin \theta' = \frac{n\lambda}{\sigma};$$

and

$$\frac{\cos^2 \theta}{r} = \frac{\cos \theta}{c} + \frac{1}{d},$$

$$\frac{\cos^2 \theta'}{r'} = \frac{\cos \theta'}{c} - \frac{1}{d},$$

where d is any quantity.

In the last equation put $180 + \theta'$ for θ' , and $-r'$ for r' . The equation then becomes identical with the corresponding equation. Hence curves whose equation is

$$\frac{\cos^2 \theta}{r} = \frac{\cos \theta}{c} + \frac{1}{d}$$

have the property that, if the source of light is at any point on one of these curves, the whole of the reflected spectra produced by the grating lie on the same curve.

If we start with the equation for transmitted light,

$$\sin \theta' - \sin \theta = n \frac{\lambda}{\sigma},$$

we shall arrive at the same result. Hence we see that each of the curves whose equation has just been found is the locus of the foci of all the diffracted rays, when the source of light is at any point on the curve.

These curves I will call "diffraction-curves." It is obvious from the equation that they are independent of the distance between the lines of the grating.

A table is given at the end of the paper showing the values of r for every five degrees in the value of θ , d having the values $\frac{c}{3}$, c , $3c$, c being taken as 1000; and the forms of the curves are shown in fig. 3.

When d is infinite, the diffraction-curve is a circle having the radius of curvature of the grating as diameter, and a straight line through D tangential to the grating. In every other case the curve is formed of two loops, one lying inside the circle and the other outside, touching one another at D. The inner loop is always an oval, which is infinitely small when d is zero, and increases as d increases, until d becomes infinite, when the inner loop coincides with the diffraction-circle. The outer loop is finite when d is less than c ; and increases as d increases, until d equals c , when the outer loop becomes infinite, and resembles a parabola. When d is greater than c the outer loop takes somewhat the form of an hyperbola, with the asymptotes inclined to the axis at an angle whose cosine is $\frac{c}{d}$, and intersecting one another at a distance from the

grating $= \frac{c^3}{d^2 - c^2}$. One of the two branches into which the outer loop is now divided passes through D, always retaining the resemblance to a branch of an hyperbola, and ultimately, when d is infinite, becomes a straight line tangential to the gra-

ting. The other branch has points of inflection, if d is greater than $\frac{3}{2}c$, in the positions for which $3c \cos \theta = d - \sqrt{18c^2 + d^2}$; and when d is greater than $2c$, this branch has points which are at a minimum distance from D. At these points the distance from D is $\frac{4c^2}{d}$, and $\cos \theta = -\frac{2c}{d}$. Consequently the locus of these points is the circle of curvature of the grating. When d becomes infinite, this branch coincides with the tangent to the grating at D, and with the diffraction-circle.

The diffraction-curve has been shown to consist of two loops, one of which passes through the source of light. This loop is the locus of the spectra of transmitted light; and the wave-length at any point is given by the equation

$$n\lambda = \sigma (\sin \theta' - \sin \theta).$$

The other loop is the locus of the spectra of reflected light; and the wave-length at any point is given by the equation

$$n\lambda = \sigma (\sin \theta' + \sin \theta).$$

As both loops coincide in the diffraction-circle, this circle is the locus both of the spectra of transmitted and of reflected light when the source of light is on the circle.

As an example of the determination of the wave-length, suppose the grating to have 25,000 lines to the inch; then each division of the grating is 40 millionths of an inch. Divide the diameter of the diffraction-circle into 40 parts, and with the centre of curvature as centre describe circles through these divisions, and number the points in which they cut the diffraction-circle, beginning with the centre of curvature as zero, and counting the readings as positive on one side of the zero and negative on the other (see fig. 4, in which only every tenth reading is given). If the source of light be at the centre of curvature, the readings of the diffraction-circle will give the wave-lengths, or multiples of them, in millionths of an inch. Now with the centre of the grating as centre of projection, project the readings of the diffraction-circle on both branches of any diffraction-curve, and place the source of light at the point in which one of the loops cuts the perpendicular from the centre of the grating. The readings give the wave-lengths, or multiples of them, as before.

Let the source of light be now placed at any point of a graduated diffraction-curve. Take the reading of the point, and *subtract* it from the readings of all other points on the *same* loop; the new readings will give the wave-lengths, or multiples of them, for transmitted light. *Add* the reading of the

position of the source of light to the readings of all points on the *other* loop, and the new readings will give the wave-lengths, or multiples of them, for reflected light. The diffraction-circle must be treated as two distinct loops, and the reading of the source be subtracted from the readings on the circle for transmitted, and added for reflected light. One of the zero-readings occurs at the source of light, and the other at the focus of ordinary reflected light.

In the case of a plane grating, since c is infinite, the equation to the diffraction-curves becomes

$$r = d \cos^2 \theta.$$

TABLE showing the values of r for every five degrees of θ in the equation

$$\frac{\cos^2 \theta}{r} = \frac{\cos \theta}{c} + \frac{1}{d}.$$

$\pm\theta.$	$d=\frac{c}{3}.$	$d=c.$	$d=3c.$	$d=\frac{c}{3}.$	$d=c.$	$d=3c.$	$\pm\theta.$
90	0	0	0	0	0	0	90
85	2	7	18	3	8	31	95
80	9	20	77	11	37	190	100
75	21	53	113	25	90	899	105
...	13691	109
70	35	87	173	44	178	-13451	110
65	52	126	231	73	309	-2000	115
60	71	167	300	100	500	-1500	120
55	92	209	363	140	772	-1372	125
50	113	252	423	175	1156	-1335	130
45	135	293	481	224	1707	-1338	135
40	156	332	535	263	2508	-1356	140
35	176	369	582	314	3710	-1382	145
30	194	402	625	351	5603	-1408	150
25	210	431	663	392	8768	-1434	155
20	224	455	694	429	14640	-1456	160
15	235	475	718	459	27394	-1475	165
10	243	489	736	481	63890	-1489	170
5	248	497	746	495	261165	-1497	175
0	250	500	750	500	∞	-1500	180

$c=1000.$

XXIV. *The Basis of Statics.* By HORACE LAMB, M.A.,
Professor of Mathematics in the University of Adelaide*.

THE object of this paper is to suggest a new basis for the science of Statics. The suggestion is put forward with all diffidence ; but it will, I think, be readily granted that the present form of the subject is hardly satisfactory.

* Read at the Annual Meeting of the Association for the Improvement of Geometrical Teaching, January 17th, 1883. Communicated by the Association at the request of the Author.

The manner in which the fundamental propositions are presented by all but the most recent writers is too familiar to need any long recital. Certain principles are assumed as the result of experience; and the parallelogram of forces and the other leading propositions of Statics are then deduced from these by purely mathematical reasoning. Conspicuous among these principles is that of the "transmissibility of force," which asserts that a force acting at any point A of a body may be supposed applied indifferently at any other point B in its line of action. This principle is usually stated with a certain amount of hesitation and qualification. We are told that it is not generally true, unless B be "rigidly" connected with A; and when, as sometimes happens, we wish for mathematical purposes to transfer the force to points not so connected, we are (in effect) told to conjure up before our minds the vision of an imaginary "rigid" framework attached to the body with which we are dealing. When the desired conclusion has been reached, this framework is conveniently dismissed to the void whence it came. Of a piece with this artifice is that which, in the application to Elasticity, Hydrostatics, &c., consists in imagining certain portions of matter to become "solidified." Now all this seems very unnatural. In the first place there are no "rigid" bodies, in the sense in which the word is defined by the writers here criticised. The fundamental propositions of Statics ought surely to admit of being stated in such a way that they shall be true, accurately and without any manner of qualification, of matter such as we find it. Again, it ought to be possible to establish the laws of equilibrium &c. of one body A without introducing, even in imagination, another body (framework) B, even if the properties attributed to B were real and not fictitious.

Since the publication of Thomson and Tait's 'Natural Philosophy' the methods here recalled have fallen into some discredit. These writers first pointed out that a sufficient appeal to experience having been made once for all in the Laws of Motion, it was unnecessary and unphilosophical to introduce a fresh set of experimental data as a basis for Statics, which is after all but a branch of Kinetics. Although the propriety of this view has been universally acknowledged, no subsequent writer has, so far as I know, ventured to follow resolutely the path thus indicated, and to develop a thoroughly consistent doctrine of Statics based solely on the Laws of Motion and their consequences. Thus in a text-book otherwise admirable we find that although the truth of the parallelogram of forces is asserted* to be an immediate consequence of

* Attention seems hardly to have been sufficiently directed to the fact

Newton's Second Law, yet the principle of the transmissibility of force is retained, and employed in deducing the rules for the composition of parallel forces, &c. In the method followed by Thomson and Tait the particular dynamical principle adopted as a basis is that of Work, in the form of Lagrange's principle of virtual velocities. This is of course the method most consistent with the plan of their book, which is to exhibit the connexion between the various branches of mathematical physics in the light of the doctrine of the Conservation of Energy. But an alternative presentment of the subject seems desirable, and, at all events for the purposes of elementary teaching, even necessary. It should be remarked, too, that Thomson and Tait follow the ancient practice of regarding Statics as a subject which deals primarily with ideal "rigid" bodies, and that accordingly, in their treatment of Hydrostatics &c., they retain (as I cannot but think most unfortunately) the artifice of an imaginary "solidification" of the portions of matter to which the fundamental propositions are to be applied.

It is with all due deference that I venture to suggest a new point of departure in a subject which has been handled by so many distinguished writers. I am of opinion that the true and proper basis of Statics is to be sought for in the principles of linear and angular momentum. Regarding Statics as the doctrine of the equivalence of forces, I would define the word "equivalent," and say that two sets of forces are "equivalent" when, and only when, they produce the same effect on the linear and on the angular momentum of any material system to which they may be applied—*i. e.* when they produce the same rate of change of momentum in any assigned direction, and the same rate of change of moment of momentum about any assigned axis. In the same way two sets of forces would be said to be "in equilibrium" when they produce no effect on either the linear or the angular momentum of any system. On this basis the fundamental theorems of Statics can be developed with great ease; and there is of course no qualification as to the physical nature of the system on which the forces are supposed to act. The particular status of *rigid*

that this assertion is only correct so long as we are dealing with forces acting on a particle. When we have to do with a body of finite size, all that the most liberal interpretation of the second law can tell us is that two forces represented by two lines AB, AC are equivalent to a force represented by some line equal and parallel to the diagonal AD of the parallelogram constructed on those lines. To prove that the resultant must act in the line AD we require the third law as well. A good deal of confusion seems to have arisen here (and elsewhere in our subject) from the vague way in which the word "equivalent" has been used.

bodies in this method is as follows. As a matter of fact we find that in many cases the bodies with which we deal undergo deformations so slight that they may for many purposes be ignored. When this is the case the fundamental principles of Statics, obtained in the above way, are *sufficient*, and enable us, in virtue of known kinematical theorems, to obtain all the information we care about. In other cases (fluids and flexible solids) these principles are equally valid, but they are no longer sufficient, and additional experimental knowledge has then to be sought for.

A possible objection to the above method may be here anticipated. It may be urged that, even granting its soundness, it is altogether too difficult and abstruse for the purposes of elementary teaching. I believe that, on examination, much of this supposed difficulty will disappear, and that, *on the whole*, the method will be found to be really much simpler than that at present in vogue. The main difficulty is at the outset. When the foundation of the method has once been laid, a whole series of propositions, now usually obtained each by a separate and often a complicated proof, follow as immediate corollaries. It is, I think, now coming to be generally admitted that some knowledge of Kinetics ought to precede the study of Statics. Now the principles of linear and angular momentum can be deduced (as in 'Thomson and Tait,' § 267) with great ease from the second and third laws of motion, and constitute of themselves a valuable intellectual acquisition. The first of these principles leads directly to the parallelogram of forces as far as the magnitude and direction of the resultant is concerned; and the second principle, with the help of Varignon's geometrical theorem of moments, fixes its line of action. The rules for the composition of parallel forces, the equivalence of couples of the same moment in parallel planes, the reduction of a system of forces in one plane, the various forms of the conditions of equilibrium, &c. need no longer to be deduced by laborious processes from the parallelogram of forces, but are simple and almost self-evident corollaries from the fundamental principles of momentum. It may be noticed, too, that the "transmissibility of force" now receives an exact and perfectly general meaning, although as a separate principle it has no proper place in our method.

It seems unlikely that the views here advocated should be altogether novel; but the only approximation to them (a remarkable one, however) which I have been able to discover is in Professor Minchin's 'Statics,' § 94 (second edition). If Professor Minchin had followed out to its legitimate conclusion the line of thought there indicated, and made it the

foundation of his exposition of the subject, he would have removed what I take to be the only serious blemish in his excellent book.

In conclusion, I would point out that the wider scope here sought to be given to the fundamental theorems of Statics can be justified on other grounds. It is now generally held that the ultimate particles of bodies apparently at rest are really in a state of more or less violent agitation. If we regard Statics as the theory of equilibrium, and if by equilibrium we mean relative *rest*, there is a manifest awkwardness in applying the principles established on this basis to bodies so constituted. On the other hand, in the form advocated in this paper, the fundamental theorems are directly applicable to such cases, without any modification whatever.

Adelaide, November 25, 1882.

XXV. *On Capillary Phenomena*. By JOHN T. RILEY, B.Sc.
(London), A.R.C.Sc.I., Demonstrator in the Physical
Laboratories of the Mason College*.

[Plate IV.]

ALTHOUGH the mathematical theories of capillary action which have been advanced by Laplace, Gauss, Poisson, and others agree in one point, which may be tested by experiment, they differ in the fundamental hypotheses. In all the theories the equation of the capillary surface is of the same form, involving a certain constant which can be determined by experiment only. They, however, differ in the manner in which this constant is made to depend on the molecular forces and the law of density of the fluid near the surface.

Laplace † supposes the density of the fluid to be uniform: he commences by considering an infinitely slender canal of the fluid perpendicular externally to the surface of a sphere, and calculates the total action of the sphere on the canal. The law of attraction is such a function of the distance that this function becomes insensible when the distance becomes sensible. He finds that the resultant attraction may be represented in the form $K - \frac{H}{b}$, where K and H are both independent of b , the radius of the sphere. He observes that K is much larger than $\frac{H}{b}$, and that K repre-

* Communicated by the Author.

† *Mécanique Céleste*, Supplément au Livre x.

sents the attraction of a plane surface, since $\frac{H}{b}$ vanishes when b becomes infinite. The expression $K - \frac{H}{b}$ also represents the attraction exerted on the canal by a segment cut from the sphere by a plane to which the canal is perpendicular, since all the sphere behind the plane is at too great a distance to have any effect. In a similar way, $K + \frac{H}{b}$ represents the action of a sphere upon an infinitely slender canal internally perpendicular to its surface.

He then applies this result to determine the action of any curved surface upon an internal column of fluid enclosed in an infinitely slender canal perpendicular to any point of this surface. If R and R' be the principal radii of curvature at this point, he obtains for the attraction the expression

$$K + \frac{1}{2}H \left(\frac{1}{R} + \frac{1}{R'} \right).$$

Gauss* uses the principle of virtual velocities. He forms an expression for the sum of the potentials arising from the mutual action between pairs of particles. This expression consists of three parts, corresponding to the action of gravity, the mutual action between the particles of the fluid, and the action between the particles of the fluid and the particles of the solid or fluid in contact with it. The condition that the system may be in equilibrium is that this expression shall have a minimum value.

Poisson† maintains that the density of the fluid is not uniform, but that there is a rapid variation of density near the surface. He obtains an equation of the capillary surface similar to that of Laplace, but asserts that K is very small instead of very great.

Many physicists, unwilling to grant the possibility of a negative pressure in a liquid, and confronted with the fact that capillary elevations and depressions occur *in vacuo* just as in the atmosphere, adopt the result of Laplace, and consider that a plane surface exerts a considerable pressure on the interior liquid. Thus in Everett's edition of Deschanel (6th ed. p. 133) we find the following statement:—"We cannot conceive of negative pressure existing in the interior of a liquid, and we are driven to conclude that the elevation is owing to the excess of pressure caused by the plane

* *Principia Generalia Theoriæ Figuræ Fluidorum in Statu Æquilibrii*.

† *Nouvelle Théorie de l'Action capillaire*: Paris, 1831.

surface in the containing vessel above the pressure caused by the concave surface in the capillary tube."

This molecular pressure, due to the plane surface, is a result only of the mathematical methods that have been employed to give an explanation of capillary phenomena. We cannot demonstrate experimentally the existence of this pressure, since, if introduced into the calculations at the beginning, it is afterwards eliminated and does not appear in the results.

In order to determine whether this molecular pressure, due to a plane surface, has any physical existence, I have made the following experiment, which I think conclusively proves that it has not, or, if it has, that it must be very small. I took a small funnel and drew out the stem so as to form a fine capillary tube which I bent twice at right angles, so as to bring the capillary limb parallel with the stem (fig. 6). The funnel was clamped to a stand, and water poured in to the level A; in the capillary limb it rose to B, the height of which I read with a cathetometer. Adopting Laplace's formula, and neglecting the atmospheric pressure, as its effect cancels out, the pressure in the liquid immediately under the plane surface at the level A is K , and that immediately under the curved surface at B is $K - \frac{H}{2} \left(\frac{1}{R} + \frac{1}{R'} \right)$, since R and R' are both negative.

In this case K cancels out; but if, without altering the level at A, we modify the surface tension, then K , which is, according to Laplace, a function of H , is altered, and takes a value K' . The excess of the pressure immediately below A over that immediately below B is now $K' - K + \frac{H}{2} \left(\frac{1}{R} + \frac{1}{R'} \right)$, instead of $\frac{H}{2} \left(\frac{1}{R} + \frac{1}{R'} \right)$ as before, and the capillary surface at B ought to move up or down according as $K' - K$ has a positive or negative value.

To test this, I held a glass rod, with a drop of ether adhering, close to the surface of water in the funnel: the surface tension was immediately diminished; but an observation with the cathetometer showed that no fall had occurred at B. Even when I dropped the ether on the surface I could detect no movement; but immediately I held the drop over the capillary tube, the surface at B began to sink, and finally fell several millimetres.

It is evident that, although H is very much diminished by the solution of the ether vapour in the capillary surface, the difference between K and K' is not appreciable, so that both must be exceedingly small or zero.

We may suppose that, while the drop of ether is held over the plane surface, the constitution of the liquid near the surface is continually changing, from the passage of the ether molecules into the upper layers.

The term K' is meant to include the whole of whatever plane-surface actions may exist, tending to produce an internal pressure. It is evident that K' ought to be considerably less than K , since H' is very much less than H under the same conditions of surface.

The equation of the capillary surface is then practically reduced to

$$p = \frac{H}{2} \left(\frac{1}{R} + \frac{1}{R'} \right),$$

where p is the hydrostatic pressure immediately below the capillary surface.

All that we know experimentally concerning the forces which produce capillary phenomena may be expressed in two fundamental propositions, from which all explanations of particular phenomena may be deduced. It has been abundantly shown, by the experiments of Plateau, Dupré, Van der Mensbrugghe, Terquem, and many others, that every liquid behaves as if its external layer were in a state of tension. This tension is uniform in all directions in the free surface; but its magnitude is modified by contact with the surface of a solid or another liquid. The existence of this surface-tension is the basis of the physical explanation of capillary phenomena. The only assumption it seems necessary to make—namely, that the variations of pressure in the parts of the liquid elevated above the level of the free surface obey the ordinary laws of hydrostatics—seems so evidently natural that it cannot be doubted.

Commencing with these data, it is easily proved, as if for a stretched membrane, that the intensity of the pressure supported by a meniscus is given at any point in the meniscus by the expression $p = \pm H \left(\frac{1}{R} + \frac{1}{R'} \right)$, where R and R' are the principal radii of curvature at that point, and H is the uniform tension across a linear centimetre in the surface.

The constancy of the angle of contact of a liquid with a given solid also follows easily from the assumption of a surface-tension; and thus armed we are able to explain any capillary phenomena.

In the January number of this Magazine, Prof. John Le Conte attempts to refer to two fundamental principles the explanation of several cases of the apparent attractions and repulsions of small floating bodies. He refers these phe-

nomena exclusively to the elastic reactions of the tense surface-film, whose form is modified by the proximity of the partly immersed solid bodies, and purposely leaves out of consideration the modifications of hydrostatic pressure. Of course it is evident that these phenomena are primarily produced by the elastic reactions of the capillary surfaces in the neighbourhood of the solid bodies; but to consider the motions as produced by a "*superior tension*" in one or more of the films, owing to a smaller radius of curvature of the film, is evidently an error of expression.

The tension is constant in every direction throughout the capillary surfaces; and it is clear that, since for each plate the angle of contact is the same on both sides, the horizontal components of the tensions will balance each other. The movements of the bodies are secondary results of the surface-tension, being produced by the modified hydrostatic pressures on their surfaces.

In the case of the compound plate of steel and glass, Prof. Le Conte's treatment has evidently led him into an error; for he says "*It is obvious that the tensile reaction can only tend to press the plates together: it cannot produce a motion of translation.*" On the contrary, as I shall show, the effect of the tensile forces is to tend to pull the plates asunder; but these opposite forces being equal in amount, no movement of the plate ensues. In order to gain a clear physical conception of the reasons for these apparent attractions and repulsions, it is necessary to consider all the forces at work; and that I propose to do in the following explanations:—

Case I. (fig. 1). Let A and B be two flat plates partially immersed in a liquid which wets both. If the plates be brought sufficiently near each other, the capillary surface QQ' will rise above the ordinary level of the liquid outside the plates. Let MN be drawn in the horizontal surface: if we neglect the atmospheric pressure, the hydrostatic pressure at this level will be zero. Passing upwards the pressure becomes negative, decreasing according to the ordinary law. There is thus a reaction of the nature of a tension between the liquid above MN and the solid surfaces in contact with it. From P to S the hydrostatic tension between the surfaces on the left of A will be balanced by the tension between the surfaces on the right; but from S to Q the tension towards the right is unbalanced by any tension towards the left. Since the angle of contact of the liquid with the solid is the same on both sides of A, the horizontal components of the surface-tensions are equal and in opposite

directions, so that they can produce no horizontal motion. The resultant force on A is thus the tension between the liquid surface SQ and the solid ; and this force tends to move A towards B. Similarly, the resultant force on B is the tension between the liquid surface S' Q' and the solid ; and this force moves B towards A. The two bodies are thus drawn together.

Case II. (fig. 2). In this case the liquid surface is depressed in the neighbourhood of both bodies, and, as before, the horizontal components of the surface-tension where the menisci touch the solids cancel out. It is at once evident that the hydrostatic pressures from S to Q and from S' to Q' on the external faces of A and B are unbalanced ; and they therefore *press* the bodies together.

Case III. (fig. 3). Here we find a depression of the surface near one plate, and an elevation near the other. The horizontal components of the tensions on both sides of A and B will respectively destroy each other: the resultant hydrostatic pressure from P to S forces A towards the left, and the resultant tension from M' to N' pulls B to the right, and the plates tend to separate.

Case IV. (fig. 4). We have now to consider the equilibrium of a single floating plate, with its surfaces so prepared that they produce different degrees of capillary elevation. It is very easy to see that, in an indefinite surface of the liquid, no movement can take place. For the potential energy of the surface cannot be diminished by any movement of the plate; and therefore we conclude that the plate must be in equilibrium, for otherwise its kinetic energy when moving would have been produced without any diminution of potential energy. But it is important to show that we arrive at the same conclusion from an examination of the effects of the capillary forces brought into play. The case does not offer any ground for objection to Laplace's theory, as Dr. Thomas Young insisted it did, nor to any mathematical theory from which the fundamental equation given previously can be deduced.

Let the surfaces of the compound plate (fig. 6) be such that we have on one side a capillary depression and on the other a capillary elevation. For the equilibrium of the plate we must have the horizontal component of tension H at Q + hydrostatic tension from S to Q = horizontal component of tension H at P + hydrostatic pressure from S to P. In order to determine the hydrostatic tension and pressure, we first find the pressure at any point in the curved surface. As R' is infinitely great, the general equation reduces to $p = \rho g z$

$= \frac{H}{R}$, where z is the height of the point considered (m' , fig. 5) from the axis of x taken in the level surface. From the differential equation of the curvature at the point m' we get

$$\frac{\frac{d^2z}{dx^2}}{\left\{1 + \left(\frac{dz}{dx}\right)^2\right\}^{\frac{3}{2}}} = \frac{1}{R} = \frac{g\rho z}{H},$$

and, integrating,

$$-\frac{H}{\sqrt{1 + \left(\frac{dz}{dx}\right)^2}} = \frac{g\rho z^2}{2} + \text{const.}$$

When $z=0$, $\frac{dz}{dx}=0$, and $\text{const.} = -H$, so that the equation becomes

$$H \left\{1 - \frac{1}{\sqrt{1 + \left(\frac{dz}{dx}\right)^2}}\right\} = \frac{g\rho z^2}{2}.$$

Introducing the angle β made by the tangent to the surface with the ordinate z , we get finally

$$z = \pm \sqrt{\frac{2H}{g\rho}} (1 - \sin \beta). \quad . \quad . \quad . \quad (1)$$

Now let the surface at P make an angle α with the surface of the plate, at Q an angle α' . The horizontal components of the tensions will be $H \sin \alpha$ and $H \sin \alpha'$ respectively. The hydrostatic tension on the plate from S to Q will be

$$\int_0^{SQ} g\rho z dz = \frac{g\rho(SQ)^2}{2} = H(1 - \sin \alpha')$$

from formula (1).

Similarly the hydrostatic pressure from S to P is $H(1 - \sin \alpha)$; supplying these values in the equation for equilibrium we have the identity

$$H = H.$$

We thus see that the resultant forces acting on the plate are two equal and opposite tensions which will tend to pull the plates asunder. In every case the identity shows that the magnitude of these tensions is just the same as if the surfaces came up perpendicular to the plates without elevation or depression.

In conclusion I would point out that the mathematical theories of Laplace, Gauss, and Poisson do no more than

account for a surface-tension. The investigations which Laplace gave of the rise of liquids in capillary tubes and between plates, and of the apparent attractions and repulsions of small floating bodies, cannot be considered as giving definite support to his theory, since the existence of the phenomena may be deduced from any mathematical theory which explains surface-tension.

All that Laplace did was to show that, with certain assumptions respecting the density of the liquid near the surface and the law of molecular attractions, he could prove that capillary phenomena would occur.

With other assumptions, Poisson also showed they would occur; but whether his assumptions or those of Laplace have a physical reality is an altogether different matter.

The Mason College, February 5th.

XXVI. *On the Horizontal Motion of Floating Bodies under the Action of Capillary Forces.* By A. M. WORTHINGTON.

[Plate V.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WITH respect to the phenomena of capillary attraction and repulsion of floating bodies, which are the subject of Mr. J. LeConte's paper in the *Phil. Mag.* of January last, I would remark that in most writings on capillarity in which the elevation or depression of liquids in contact with solid plates is discussed, attention is confined to the *vertical* component of the surface-tension at its contact with the solid, and to its relation with the weight of liquid elevated or depressed. The *horizontal* component is generally neglected as not affecting the results; and the very interesting relation which exists between its variation and the resulting hydrostatic pressure or tension is overlooked. The consequence of this is that the explanations which are afterwards given of the horizontal motions of floating bodies are expressed in terms which apply only to each special case as it is dealt with, and do not reveal the important underlying relationship to which I refer.

This relationship is indeed made use of by Quincke in treating of flat drops and bubbles; and he deduces it from the differential equation to the surface by a rather long process. It does not seem to have occurred to any one that the same relationship underlies the attraction and repulsion of plates, or that it can be very easily deduced from the well-established

principles of (1) a definite surface-tension, (2) a definite contact-angle, (3) a hydrostatic pressure within the liquid increasing continuously with the depth and having the value zero at the level of the free horizontal surface, so that any liquid raised above this level is in a state of tension.

We will consider the forces acting on one surface of a vertical floating plate so broad that it is not necessary to take into account the action at the edges. We will take first the case in which the liquid is depressed at its contact.

Fig. 1 represents this case. The level surface $F G$ is depressed to E .

Produce the horizontal surface $F G$ to meet the plate in K .

It is easy to show that the sum of the horizontal forces acting on the plate is precisely the same as if the angle of contact were a right angle and the surface were not depressed, but left the solid at K .

We need only consider points above the level of E ; and our remarks will have regard to a strip of the surfaces whose width in the direction perpendicular to the plane of the paper is one unit. Let T be the surface-tension per unit of length.

If the liquid were not depressed, the plate would be pulled to the left by a tension T , applied at K , and would be pushed to the right by the hydrostatic pressure due to the depth of liquid $K E$.

The surface-layer between G and E may be regarded as a smooth, weightless, perfectly flexible coherent sheet. The hydrostatic pressures due to the depth of liquid between K and E are applied everywhere at right angles to this surface, and may be resolved into vertical and horizontal components. The vertical components may be regarded as applied to the solid at E where the surface-sheet on which they act is attached to the solid.

The sum of these vertical forces is obviously equal to the weight of the liquid which would fill the space $G K E$; and the solid is accordingly buoyed up precisely as if it were so shaped as to displace this liquid, and the hydrostatic pressure were exerted on the solid itself.

In the same way the sum of the horizontal components may be regarded as applied to the coherent plane surface $F G$ at G , whereby the effective pull of the free horizontal surface on the solid is diminished by the precise amount of the hydrostatic pressure in question; so that *the result of the depression of the surface is to diminish the hydrostatic pressure on the plate to the right by precisely the same amount as the surface-pull to the left is diminished.*

Consider next the case of a plate of such a nature that the liquid is elevated. This case is represented by figure 2; and we can deal with it in exactly the same way.

The surface-layer between G and E may again be regarded as a smooth flexible coherent sheet; and it sustains a hydrostatic tension due to the liquid elevated above the surface and equal in amount to the weight of the liquid that fills the space G K E. The surface-sheet is attached to the solid at E, which is therefore weighed down by a force equal to this weight, and which is the vertical component of the surface-tension at E. The effect of the horizontal components of the hydrostatic tension exerted on the surface-sheet E G is obviously equal to that exerted on the solid in the opposite direction; so that while the effective pull of the free horizontal surface beyond G is diminished by this amount, an equivalent hydrostatic tension is substituted. Hence, *in either case, whatever the value of the contact-angle, the total horizontal force is the same as if there were no capillary elevation or depression of the surface.*

It is to be observed also that the reasoning we have just made use of is applicable to any portion of the liquid surface of unit width enclosed between horizontal lines parallel to the plate, and that the difference of the horizontal components of the surface-tensions at the two lines is equal to the horizontal hydrostatic pressure or tension on the surface between them.

If the distance from the free horizontal surface of the nearer bounding line be H, and of the further $H + y$ (perpendicular distances being reckoned positive when measured away from the free horizontal surface), and if D be the weight of unit volume of the liquid, then the horizontal hydrostatic force in question, being equal to that on a vertical rectangle of unit width and height y and whose centre of gravity is at a distance $H + \frac{y}{2}$ from the free surface, is

$$\left(H + \frac{y}{2}\right)yD;$$

and if we designate by θ_0 the acute angle between the surface and the horizontal at the level H, and by θ the corresponding angle at the level $H + y$, we may write the equation to the surface

$$\tau \cos \theta_0 - \tau \cos \theta = \left(H + \frac{y}{2}\right)yD^*,$$

the origin being taken in the surface at the level H, where

* It is a special case of this result that Prof. Quincke makes use of in his treatment of large flat drops and bubbles. It may be deduced from

$\theta = \theta_0$. With the origin at the level of the free horizontal surface the equation becomes

$$\tau \cos \theta_0 - \tau \cos \theta = \frac{y^2 D}{2}.$$

We will now apply the principle to all possible cases. Of the case of a single floating plate, whose sides are of different materials so that the liquid is raised at one side and depressed or less raised at the other, the explanation is obvious. There can be no horizontal displacement, since the total horizontal force on either side is the same as if there were no capillary elevation or depression at all.

Consideration, however, of the position of the points of application of the surface-tension, and of the centres of hydrostatic pressures or tensions, shows that the plate will tend to topple towards the side on which the liquid is elevated or more elevated.

When two parallel plates are concerned, we see then that the nature of their exterior surfaces is immaterial, since we may always imagine the exterior meniscus replaced by a plane horizontal surface.

If the interior surfaces are such that the liquid would be (a) raised or (b) depressed by either alone, then, whatever the value of the interior contact-angles, the lowest portion of the meniscus (as in fig. 3), or the highest portion (as in fig. 4),

the usual form of the differential equation to the surface very shortly as follows.

This equation referred to an origin at an elevation H above the free surface is

$$\tau \left(\frac{1}{\rho} + \frac{1}{\rho'} \right) = -D(H+y),$$

and becomes

$$\frac{\tau}{\rho} = -D(H+y)$$

when one curvature vanishes, as in the case under consideration. Writing for ρ its value

$$\frac{ds}{d\theta} = \frac{dy}{\sin \theta d\theta},$$

we get

$$\tau \sin \theta d\theta = -D(H dy + y dy),$$

or

$$\tau \cos \theta = C - \left(H + \frac{y}{2} \right) y D;$$

and when $y=0$,

$$\tau \cos \theta = \tau \cos \theta_0 = C;$$

$$\therefore \tau \cos \theta_0 - \tau \cos \theta = \left(H + \frac{y}{2} \right) y D.$$

will be horizontal, and we may replace the horizontal effects of the interior meniscus by that of the horizontal tangent plane. There then remains to be considered only the hydrostatic tension (fig. 3) or pressure (fig. 4) due to the liquid between the free horizontal surface and the tangent plane, by which, in either case, the plates will be urged together. The amount of the action is known when the elevation of the tangent plane is known.

In the remaining case, in which the nature of one plate (A) on the interior side is such that if it alone were present the liquid would be depressed, while at the other plate (B) it is raised, the difficulty lies, not in applying the principle we have obtained, but in previously ascertaining the configuration of the liquid between the plates to which we have to apply it. It will be observed that in the cases just dealt with a knowledge of this configuration has been taken for granted.

It is easy, however, by following in the steps of Laplace, and more particularly of Poisson, to obtain this preliminary information.

If the acute angle of contact ω at the plate A is *equal* to the corresponding angle ω' at the plate B, then there must always be a line F of contrary flexure in the surface midway between the two plates whatever their distance apart; and this line must be at the level of the exterior free surface, since at this level there is no hydrostatic pressure, and therefore no curvature of the surface. When the plates are far apart the surface at F is horizontal, and there will be neither attraction nor repulsion of the plates. When the plates are near together the surface will be inclined at F (as in fig. 5); and from what has been said we know that the hydrostatic effect of the depression and elevation on either side of F is exactly such as to counterbalance the diminution of the horizontal component of the surface-tension at F, so that the plates may be regarded as drawn together *by that component*, whereas they are drawn apart by the full amount of the surface-tension. Consequently they recede from each other. There is no residual hydrostatic force involved.

(The apparent repulsion of a wetted floating object from the edge of a glass of water filled above the brim is a common instance in which the obliquity of the surface at one side of the object is very obvious and the effect very striking.)

If one of the acute angles of contact, say ω , at the plate A (fig. 6) be greater than the other, ω' , at the plate B, then, if we begin with the plates far apart, we see that, as in the last case, there will at first be neither attraction nor repulsion, but that the line of contrary flexure F must be nearer to the

plate A than to the plate B (for the surface must be symmetrical on opposite sides of this line). It is also evident, on account of this symmetry, that if, keeping B fixed, we could transfer the plate A to the position A' at an equal distance at the other side of the point F, the surface of equilibrium GE' would be the same, and that there would again be no tendency to horizontal motion; for, K'E' being equal to KE, the hydrostatic tension on the plate A' would counterbalance the diminution of the horizontal component of the surface-tension. If the plate A' were now pushed nearer to B, the horizontal component of the surface-tension between them would remain the same, owing to the constancy of the contact-angles, while the hydrostatic tension would increase as the liquid rose between the plates and the curvature of its surface increased. Consequently, at all distances less than that referred to there will be attraction. (The case is indeed the same as that of fig. 3, with the difference that the lowest element of the meniscus is inclined to the horizontal.)

If, on the other hand, the plate A be withdrawn from the position A', the liquid between the plates will fall and the hydrostatic tension diminish, while the horizontal component of the surface-tension will remain the same till the line of contact with the plate A reaches the level of the free exterior surface, when it must become the line of contrary flexure, to which in all its positions we give the name F. Up to this position the repulsion will accordingly increase; after this it will diminish, since the inclination to the horizontal at F will diminish as A recedes further.

I am, Gentlemen,

Your obedient servant,

Clifton College, Bristol,
Feb. 10, 1883.

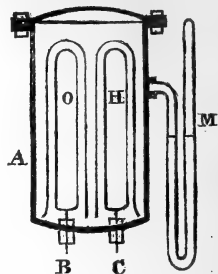
A. M. WORTHINGTON.

XXVII. *A High-Pressure Electric Accumulator or Secondary Battery.* By FREDERICK JOHN SMITH*.

THE modification of the gas-battery of Sir W. Grove which the author has devised is as follows:—The gas-battery of Sir W. Grove, as usually made, when used as an accumulator, owing to the small quantity of gases that it holds, soon runs down when in use. In order to make its life longer, the author puts the gases under a high pressure; by this means a large quantity of gas can be stored in a suitably constructed instrument. The earliest form of battery, and

* Communicated by the Author.

this has been at work (*i. e.* has been charged and discharged as an accumulator) for eighteen months, is shown in the cut. A is a strong lead vessel, well lined with rubber varnish, to prevent any solution of the lead being formed. O, H, are platinized platinum cylinders, in inverted tubes held in their proper position by pieces of rubber. M is a manometer. The terminals B, C are brought through insulating stoppers. A 10-per-cent. mixture of sulphuric acid and water is used as the liquid. With this arrangement a pressure of seven atmospheres can be easily used; and thus the tubes, one of which has twice the capacity of the other, can contain 64 times the gas that they would do at the usual pressure. A second form has been made by Messrs. Becker Co., for the author. In this a U-shaped glass tube is used, the manometer being attached to the bend, and the sheets of platinum being fused into each leg. This form, although well suited for lecture-purposes, will bear but a small pressure. A curious point observed, and one that is now being carefully worked out, is that the E.M.F. appears to vary much with the pressure.



The author has, in addition to the experiment described, charged small Faure or lead secondary batteries under pressure, the result being that the life of the battery is lengthened by being charged under pressure. Sufficient data have not as yet been collected concerning the variation of the E.M.F. under pressure, and the behaviour of the lead battery under pressure, to put into print. Should others experimentalize in the direction of high-pressure accumulators, a caution must be added with regard to the air-space left in the lead chamber: that quantity of air must be left which may be compressed, say, to seven atmospheres of pressure before the gas in O rises outside.

Oxygen produced by electrolytic action almost instantly acts on the best India-rubber tubing or varnish, causing splits and cracks to be formed in it. The chambers are now made of lead to which a harder metal has been added.

Taunton, Feb. 15, 1883.

XXVIII. *On Magnetomotive Force.*

By R. H. M. BOSANQUET, *St. John's College, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following paper is an attempt to develop the analogy between magnetism and the voltaic circuit, which was enunciated by Faraday. The assumptions of this theory are generally admitted to be true; but they have not, so far as I know, been consistently pushed to their consequences.

It appears to me, further, that this point of view is the only one for which there exists any experimental evidence. The fundamental assumptions of Poisson's theory are admittedly false; and it is only by the introduction of fictitious quantities that the existing mathematical theory has been rendered in any degree capable of representing the facts.

Faraday compared a magnet to a voltaic battery immersed in water*; and he established by experiment the principal analogies on which this comparison is founded.

The first principle of the voltaic circuit is, that the current produced by a given electromotive force in a circuit depends on the resistance of the circuit as a whole.

I shall use the expression "magnetomotive force" to indicate the analogue of electromotive force. It is a difference of magnetic potential, just as electromotive force is a difference of electric potential.

Now the fundamental hypothesis at the base of the ordinary mathematical theory of magnetism is, that there are magnetizing forces \mathfrak{H} which are of the dimensions of the magnetic induction \mathfrak{B} which they produce, and that the magnetizing force permeates every medium, and produces in magnetic media magnetic induction proportional to the force and to a coefficient of permeability μ , quite independently of the existence of any magnetic circuit.

This is the simplest way of putting it. I will state the case presently in terms of the quantity known as "magnetization," which is the quotient of moment by volume.

There are two objections to this. First, in relation to complete circuits.

Consider a sphere of iron, or a disk magnetized normally to its plane. Then (Maxwell, ii. p. 66) the magnetic induction through it is small. Now let it form part of a bar, and let the bar be bent round into a ring, so as to establish a magnetic circuit. We know that the magnetic induction through the same piece under the same force will become enormous. Can

* Experimental Researches, iii. p. 424, par. 3276.

it be said that it is a natural expression of such facts to assume that the magnetism depends on the force and on the conductivity of the material of disk or sphere, leaving the existence of the circuit out of the question? It would be equally sensible to take a piece of copper out of a voltaic circuit, and found our theory of current electricity on the hypothesis that an electromotive force acting on the copper produced a current through it proportional to its conductivity and to the force, irrespective of the completion of any circuit.

There is in magnetism this circumstance, which gives somewhat more colour to the hypothesis than there would be in the above case—namely, that space conducts magnetism much better than it does electricity; and in consequence *some* magnetic induction can always be set up in iron by a magnetomotive force—just as, if we lived in the sea, and had voltaic batteries and dynamo machines there, the action of an electromotive force would always produce *some* current, the circuit being completed by the conducting-power of the sea-water.

The second objection is to the substitution of the so-called “magnetizing force” for a magnetomotive force. This is just as if, living in the sea, we associated electromotive forces with the currents in the sea-water which would inseparably accompany them, took these currents for the measures of the forces, and called them the electrizing forces.

In carrying out the ordinary theory on this basis, we have to suppose that the magnetizing force \mathfrak{H} within a magnetic body has the power of remaining separate and distinct from the magnetic induction as a whole, though the two are quantities of the same nature. This has always seemed to me to present insuperable difficulties as a physical conception.

So soon as we replace the “magnetizing force” by a difference of potential or magnetomotive force, we can assimilate the whole conception to that of the origin of an electric current under an electromotive force. The quantity \mathfrak{H} becomes merely the magnetic induction in vacant space, and \mathfrak{B} that in magnetic matter. \mathfrak{B} replaces \mathfrak{H} , and is not supposed to include it as before. According to the ordinary theory,

$$\mathfrak{B} = \mathfrak{H} + 4\pi\mathfrak{I} \quad \text{or} \quad \mu = 1 + 4\pi\kappa;$$

where

$$\mu = \frac{\mathfrak{B}}{\mathfrak{H}}, \quad \kappa = \frac{\mathfrak{I}}{\mathfrak{H}}, \quad \mathfrak{I} = \frac{\text{moment}}{\text{volume}}.$$

The change of conception and the real meaning of the formula can be shown as follows:—

Let $\mu = 1 + \lambda$. Suppose an infinite* bar acted on by a mag-

* The bar must be infinite, not merely long. Rowland found the influence of the ends still sensible in the longest bars he tried.

netizing force \mathfrak{H} . The old theory says there are all together $\mu A \mathfrak{H}$ lines of force (A = section of bar), and

$$\mu A \mathfrak{H} = A \mathfrak{H} (1 + \lambda), \quad (1)$$

where $A \mathfrak{H}$ are the lines of force of the magnetizing force itself, $A \mathfrak{H} \lambda$ those added by the induction.

These last form the poles. And, since there are 4π lines of force round a unit pole, strength of pole $= \frac{\lambda A \mathfrak{H}}{4\pi}$.

Again,

$$\begin{aligned} \text{Moment} &= \text{pole} \times \text{distance of foci}, \\ (\text{ultimately}) &= \text{pole} \times \text{length of bar}, \\ &= \frac{\lambda \mathfrak{H}}{4\pi} \times \text{volume}; \end{aligned}$$

and

$$\mathfrak{J} = \frac{\text{moment}}{\text{volume}} = \frac{\lambda \mathfrak{H}}{4\pi}.$$

Substituting for $\lambda \mathfrak{H}$ in (1),

$$\mu \mathfrak{H} = \mathfrak{H} + 4\pi \mathfrak{J};$$

whence come the equations of the ordinary theory first above written.

From our point of view $\mu = \lambda$ in the above, the action of the magnetic matter replacing that of space instead of being added to it; and our fundamental equation becomes

$$\mu = 4\pi \kappa, \text{ or } \mathfrak{B} = 4\pi \mathfrak{J}.$$

I believe that there is no evidence whatever for the view that represents \mathfrak{H} as subsisting independently throughout the magnetic body.

If we are really to carry out Faraday's theory of magnetism, we must take into account the entire resistances of the circuits formed by iron and air, and then determine the magnetic induction through the circuit as the quotient of the "magnetomotive force" by the total resistance.

We may define the unit of "magnetomotive force" as that which, acting through a unit of magnetic resistance, produces a unit of field-intensity or magnetic induction.

Consider a solenoid having its ends joined. Then, if the resistance unit be that of 1 centim. of the length in air, x is the resistance of length x of the solenoid. Similarly, if the solenoid be filled with an iron ring of permeability μ , x/μ is the resistance of length x of the ring. And if x_0 be the whole length of the ring, M the whole magnetomotive force,

$$\frac{M}{\frac{x_0}{\mu}} = \mathfrak{B},$$

or

$$M = \frac{x_0}{\mu} \mathfrak{B} = x_0 \mathfrak{H},$$

where M is the whole difference of magnetic potential which acts on the induction as it traverses the circuit once.

If C be the current in the coils, n the number of coils,

$$M = 4\pi Cn,$$

since the point considered has gone once round each spire of the coil.

Put $M=1=4\pi Cn$. Then, if we put $n=1$,

$$C = \frac{1}{4\pi};$$

and the C.G.S. unit of current is 10 ampères;

$$\therefore C = \frac{10}{4\pi} \text{ ampères, } = \cdot 8 \text{ ampère nearly.}$$

Hence the unit of magnetomotive force is that which acts on a circuit singly linked with one spire of a current of $10/(4\pi)$ ampères. Thus a soft-iron horseshoe with ends nearly meeting, round which a wire carrying such a current is wound once, would exhibit nearly the unit of magnetomotive force between its poles. (See *post*, on broken circuits.)

Example of a ring solenoid.—Let the length of the solenoid round the axis be 100 centim.,

Current = $10/(4\pi)$ ampères, number of coils = 1000;

$$\therefore M=1000;$$

and

$$\frac{M}{x} = 10 = \mathfrak{H} = \text{intensity within the ring in air.}$$

The area of the section of the resistance comes in as a factor on both sides. Strictly the unit resistance would be that of 1 centim. length of an air-cylinder whose cross section has an area of 1 square centim. If we suppose the area of the section of the air-space enclosed in the solenoid to have this value, its radius would be $1/\sqrt{\pi}$ centim.

In general all the lines of force pass through some one section, generally the equatorial section of a bar, so that the total magnetic induction is the product of the magnetic induction through unit area and the area of this section. It is usually convenient to express the resistance in terms of the length of a cylinder having the same sectional area. This area appears on both sides, and may be struck out.

Suppose the above ring-shaped solenoid to be wound about

an iron ring whose permeability $=\mu$. Then

$$\frac{\frac{M}{100}}{\mu} = \mu \cdot 10 = \mathfrak{B}$$

gives the magnetic induction in the iron. It was by measuring this quantity in rings that Rowland determined the values of μ under different magnetic inductions (Phil. Mag. xlv. p. 140).

Now it is possible from the above equation, supposing one of Rowland's tables to be correct, to tell what the magnetic induction and permeability for soft iron would be in the above case. The induction is ten times the permeability; we have therefore only to find the corresponding point in Rowland's table for soft iron, reduced to C.G.S. measure. I take the liberty of transcribing the two columns required from Phil. Mag. xlv. p. 151. \mathfrak{B} is Rowland's Q reduced to C.G.S. by dividing by 10.

\mathfrak{B} .		μ .		\mathfrak{B} .		μ .
71.5	390.7		7473	2367
600.5	868.7		8943	2208
966.7	1129		10080	1899
2460	1936		12270	1448
2923	2078		12970	1269
3082	2124		13630	1137
4959	2433		14540	824.1
5482	2470		15770	461.8
5782	2472		16270	353.8
6651	2448		16600	258.0
				17500	0

The pair of values most nearly corresponding to $\mathfrak{B}=10\mu$ is $\mathfrak{B}=12970$, $\mu=1269$.

The observation that μ as a property of the iron must be a function of its condition, and probably of \mathfrak{B} , was made by Rowland. We see that it cannot possibly depend on \mathfrak{H} as is usually supposed, as this is simply the field-intensity produced in air by the given magnetomotive force, and has nothing to do with the iron.

In the present case (iron ring) \mathfrak{H} is the numerical ratio of \mathfrak{B} and μ , and so a function of the iron; but, in broken circuits into which air-resistances enter, μ , the permeability of the circuit, depends in general chiefly on the air-resistances. So that, for given values of \mathfrak{H} , the functions of the iron may have widely differing values, according to the value of the external resistance of the circuit.

As another example we may suppose the current in the last

case reduced to $\frac{1}{10}$ of its amount—*i. e.* to .08 ampère say. Then $M=100$, and $\mu=3$. This points to a value between the third and fourth steps of the table, where each of these functions would be about 1250.

Let us now suppose the ends of the solenoid separated, and apply the analogue of the law which regulates the E.M.F. between the terminals of a battery.

If R be the internal resistance of the battery,

r the external resistance,

E total E.M.F.,

e E.M.F. between terminals;

then

$$e = \frac{r}{R+r} E.$$

Similarly let X be the internal resistance of a magnetic solenoid,

x the external resistance,

M the total magnetomotive force of the circuit,

m the magnetomotive force between the ends of the solenoid;

then

$$m = \frac{x}{X+x} M.$$

Here we assume that the whole magnetomotive force acts within the solenoid. This is not strictly true; for every part of the circuit is subject to some portion of it; but it is nearly enough true for approximate purposes.

This is generally in accordance with fact (see Faraday, Exp. Res. iii. p. 428, par. 3283). The case of the soft-iron horseshoe surrounded by one or more coils of wire may now be considered. X will be small, and x , the air-resistance, great; $\therefore m$ nearly $= M$, as was observed in speaking of the unit magnetomotive force. If, on the other hand, an armature be applied having a resistance x much less than X , the free magnetomotive force at the terminals is reduced, or m becomes a small fraction of M . Other cases can be discussed in the light of the analogy of the voltaic circuit. The solenoid without iron corresponds to a battery of high internal resistance; it may be regarded as joined up through the comparatively small resistances at the end, and presents but little free magnetomotive force.

Let us now consider the case of a body of great conductivity exposed to a uniform magnetic field, such as that of the earth's horizontal magnetism. It is clear that, in consequence of the conductivity, the potential at the ends of the conductor tends to be equalized; and if the conductivity were infinite it would be equalized throughout the body. The whole of the body must therefore be regarded as being at the potential which in

its absence its centre of symmetry would have had. The fall of potential on approaching the body is greater than in the undisturbed state. This gives rise to stream-line problems, which are the same as those ordinarily dealt with.

We see that, in such a field, no new lines of force can be developed in any circuit; for the action of the uniform magnetomotive force on the opposite portions of the circuit is the same in amount and opposite in direction.

We may use the known solutions to obtain the permeability of a sphere, by which we mean the ratio of the number of lines of force through its equatorial section to the number through the same section in air. This is 3 for a sphere of infinite conductivity. This is deduced by Stefan in a recent number of Wiedemann's *Annalen*, xvii. p. 956. It can also be obtained from fig. 4, p. 489, of the Reprint of Sir William Thomson's Papers, by comparing the square of the ordinate of the outside line inflected so as just to meet the sphere with the square of the radius of the sphere. This gives

$$(1.375 \times \sqrt[3]{2})^2 = 3.001.$$

In both these cases the solution only refers to the case of a uniform field of infinite extent, which excludes circuits (as remarked above). I shall presently examine this excluded case.

In the meantime an important point may here be noticed. If we calculate the *magnetization* of a sphere of infinite conductivity by the usual formula (Maxwell, vol. ii. p. 65), we obtain the number $3/(4\pi)$. Now if we seek to deduce the permeability from this by the usual formula $\mu = (1 + 4\pi k)$, we find the number 4 instead of 3 as given by the above investigations. This obviously arises from the formula being based on the hypothesis that the "magnetizing force" penetrates unchanged through the body, and is to be added to the distribution of stream-lines which has been determined. It is very difficult to admit this. Our point of view, according to which the magnetizing agent is a magnetomotive force and not a field intensity, removes this difficulty; and the formula for μ reduces to $\mu = 4\pi k$, which gives 3, as before, in the present case.

We can obtain a more general approximate solution for the case of a sphere subjected to a magnetomotive force such that the sphere forms part of circuits through which the force acts, in a form suitable for experimental verification.

Let a coil be wound on a reel having a cylindrical opening within. Length of reel = diameter of sphere = diameter of cylindrical opening. Then magnetic circuits will be formed passing through the sphere and linked once with each turn of the coil. It will be near enough for the present purpose to assume that the lines of force radiate at right angles from the

surface of the sphere in all directions. This is the case close to the surface; and by far the greater portion of the resistance of the divergence arises close to the surface. It is, then, easy to show that the resistance of the divergence from each hemisphere is equal to that of a cylindrical air-space having the equatorial section of the sphere for base, and height = half the radius. In fact, if $s=2\pi r^2$,

$$\begin{aligned} \text{resistance of hemispherical shell} &= \frac{dr}{s}, \\ \text{total resistance} &= \int_a^\infty \frac{dr}{2\pi r^2} = \frac{1}{2\pi} \left[-\frac{1}{r} \right]_a^\infty = \frac{1}{2\pi a} \\ &= \frac{a}{\pi a^2}. \end{aligned}$$

If the sphere be of infinite conductivity, the total resistance is twice this, *i. e.* a cylinder of altitude a .

Remove the sphere. Then the resistance is that of the cylinder, with divergence from the flat ends. If we take these divergences each to have resistance $\cdot 6 a$ of the cylinder, as we know to be the case approximately in the analogous case of the divergence of sound from the end of a pipe*, we have for the whole resistance in this case that due to cylinder + 2 ends, measured by $a(2+2 \times \cdot 6)=3\cdot 2 a$. Comparing this with the resistance of the sphere, which was measured by a , we have 3·2 for the permeability of the sphere, which agrees very fairly with what went before.

There remains the important case of a disk. According to our view the disk will have finite air-resistances around it, and when its thickness becomes small the air-resistances will not be sensibly altered by its removal. The conductivity of the circuit through a thin disk is therefore unity. According to the ordinary theory (Maxwell, vol. ii. p. 65), the *magnetization* of a disk for which $\kappa=\infty$ is $1/(4\pi)$. Here we meet again the same difficulty as in the case of the sphere: if we use the ordinary formula $\mu=1+4\pi k$, and assume that the *magnetizing force* flows through the disk as well as the lines of force that result from the magnetization, then $\mu=2$. But from our point of view the force is a magnetomotive force; the induction in the substance takes the place of that in space, and is not additional to it, and $\mu=1$, or the state of things is unaffected by the disk.

Permanent Magnets.

The following quotations appear to embody the facts as they are supposed to be:—

* See Lord Rayleigh on Sound, ii. p. 169.

(1) Maxwell, vol. ii. p. 45 :—" If a magnet could be constructed so that the distribution of its magnetization is not altered by any magnetic force brought to act on it, it might be called a rigidly magnetized body."

(2) Gordon, 'Electricity and Magnetism,' i. p. 148 :—" Into however many pieces we cut a magnet, each will have two opposite poles, whose strength is equal to that of the poles of the original magnet." P. 151—" The moment of a magnet is not altered by cutting it in pieces." P. 155—" If from any magnetized substance we cut any piece whatever, its magnetic moment is simply proportional to its volume."

Now these two hypotheses may apply to a theoretical magnetism which can be imagined; but they are both far from representing the actual behaviour of permanent magnets.

With respect to (1), I shall develop a hypothesis which leads to an account of the properties of permanent magnets at all events nearer the truth than (1). (2) is very far from being true.

(1) After this passage Maxwell proceeds:—" The only known body which fulfils this condition " (rigid magnetization) " is a conducting circuit round which a constant current is made to flow. Such a circuit exhibits magnetic properties, and may therefore be called an electro-magnet; but these magnetic properties are not affected by the other magnetic forces in the field."

Now from the point of view of the preceding investigation, we should not call the current-circuit a rigid electromagnet. We should speak of it as possessing a definite magnetomotive force, and say that it magnetizes the space or other objects in its neighbourhood. And this magnetism is by no means rigid, but depends on the resistance (or on the permeability) of the magnetic circuits through which it flows; *i. e.* it is modified by the introduction of iron into the field, which rigid magnetism would not be.

If, then, the hypothesis of frictionless Ampèrian currents in magnets be at all correct, even as an analogy, the first supposition as to the nature of permanent magnets will be that they possess a constant magnetomotive force in their substance in virtue of the Ampèrian currents; or this may be simply assumed without further hypothesis. And this magnetomotive force maintains a magnetic induction in the magnet which depends on the total resistance of the magnetic circuit.

Now, if this be true, suppose a long steel bar-magnet to be cut up into short pieces. The resistance of the whole consists of two parts—that of the steel, and the air-resistances at

the two ends ; there are also parallel air-circuits which start off all along the sides of the rod. These latter are under smaller differences of potential than the divergences at the end, and may, in the first instance, with rods not very long, be neglected in comparison with the resistances at the ends.

Now suppose the rod cut up into n pieces : we have the same total magnetomotive forces, the same total steel resistance, and the resistance of $2n$ ends. The magnetic induction in each piece is therefore altered in a ratio which depends on the ratio of the steel-resistance of one of the little pieces to the air-resistance of its two ends.

It is not possible to cut up a hard steel bar without disturbing the magnetism ; in fact it is hardly possible to cut it up at all. I have therefore preferred to cut a soft steel bar into short lengths, finish these as accurately as possible so that they may be put together to form one long bar ; then harden, glass-hard ; then grind the ends with emery and oil till the pieces will pick each other up when firmly pressed together with a trace of oil on the faces ; then magnetize.

The compound bar is then suspended in a cradle by means of a bifilar suspension arranged with its equilibrium-plane at right angles to the magnetic meridian. If it were true that the moments of the separate portions were the same whether joined up or not, the deflection should be the same in both cases. But it is not so. The deflection when the bar is joined up and pressed together is many times as great as that obtained when the pieces are so dispersed about the tray which carries them as to be fairly removed from each other's influence.

A rough preliminary arrangement showed the existence of a difference, but did not lead to the detection of the smallness of the effect produced by the separated pieces.

A bifilar suspension was then constructed. It has a pulley for the wire to pass over ; adjustable slides with metre- and inch-scales carrying the holes for the wire to pass through ; and a circular seat, with a circle divided to degrees ; this is fixed on a firm crossbeam at a good height. The wires enter, through a hole in the cover, a cylindrical case, whose sides are made of narrow pieces of flat glass. Within this case swings the cradle which carries the magnet. It has three V-shaped troughs. The pieces can either be wedged together in the middle trough, or be placed at considerable distances in all three. The cradle is 16 inches long. It is suspended over a circle divided to degrees and having nearly 16 inches diameter. Pointers are attached to the ends of the cradle, which play in front of the circle. There is not more than $\frac{1}{16}$ of an inch to spare between points and circle on

each side, so that the centering has to be very true for the cradle to swing free. The circle is read through the glass sides of the case.

The magnet employed is made from a cylindrical bar of cast tool-steel. It consists of eighteen pieces, fitted and hardened as above described. They were then fastened together in two lengths of 9 pieces each with wooden splints, and placed between the terminals of an electro-magnetic magnetizer constructed for the purpose. When screwed up firmly between the poles of the magnetizer a current was transmitted through the coils. In this condition it was tapped with a hammer for some time. When removed, each compound bar retained a considerable permanent magnetism.

The chief difficulty was to observe with sufficient accuracy the small deflection produced by the separated pieces. Small differences in this small quantity produce large differences in the calculated resistance (or permeability) of the steel. The measures now given probably reach to an accuracy of about a tenth of a degree. Up to this point I have endeavoured to take count of all the errors of the apparatus. The final measures are:—

Deflection due to 18 pieces joined up = $13^{\circ}0$
 „ „ separated = $1^{\circ}05^*$.

Dimensions of magnet:— centim.
 Whole length = 28.50
 Length of each piece = 1.58
 Diameter = 1.97 = 2R

Let r be the resistance of one of the steel pieces expressed in centimetres of a similar air-cylinder,

αR the resistance of one end ;

then
$$\frac{18r + 2\alpha R}{18r + 18 \times 2\alpha R} = \frac{1.05}{13} = .08 \text{ nearly}$$

(assuming the forces proportional to the deflections).

Then $r = .053 \times \alpha$ centim.

and length of piece = 1.58 centim.

$$\therefore \text{ratio of } \frac{\text{length of air}}{\text{steel of same resistance}} = \frac{.053 \times \alpha}{1.58} \\ = .034 \times \alpha.$$

* After the pieces had remained separated for some days in the bifilar, I noticed that the reading had changed in the direction of increased moment. A set of readings gave $13^{\circ}0$ and $1^{\circ}9$. After standing for some days joined up again, $13^{\circ}2$ and $1^{\circ}8$. These latter values correspond to $\mu=15$ nearly if $a=.6$, to $\mu=29$ nearly if $a=.3$. There appears to have been a sort of spontaneous rearrangement of the magnetism of the little pieces in the direction of less resistance, probably with diminution of a . The spontaneous change was an increase of moment, not a diminution. The point requires further examination.

If we assume $\alpha = \cdot 6$ from analogy to sound and electricity,
ratio = $\cdot 020$.

α is not likely to be greater than this ; it may be less, as the case is like that of a tube with side resistances removed to a certain extent.

The conclusion is : The magnetic induction of a permanent magnet may be supposed to be produced by a magnetomotive force derived from permanent Ampèrian currents, acting through the resistance of the steel.

In the case of the steel examined, this resistance was $\frac{1}{50}$ of that of space, if $\alpha = \cdot 6$. If α be less the resistance will be less in proportion.

Meyer (Wiedemann, *Ann.* xviii. p. 233) has determined the magnetization-function k of hard steel (p. 245). He finds generally values varying from 2 to 3 for small magnetizing forces, but in some cases as much as 9 or even 12. These correspond, according to our formula $\mu = 4\pi k$, to the following values of μ :—

$k \dots 2$	3	8	12
$\mu \dots 25\cdot 1$	$37\cdot 7$	$100\cdot 6$	$150\cdot 8$

According to our result μ would be 50 for the above steel. The number is quite uncertain, as the application of the coefficient α is as yet hypothetical. But it shows that the hypothesis is not in contradiction with known values. Determinations of all the quantities involved require instruments of a more accurate kind than those I have hitherto employed. These are being constructed for other purposes ; and I hope to examine the matter further.

The assumption of the existence of magnetomotive force and resistance in permanent magnets appears to be the necessary consequence of Faraday's comparison of the permanent magnet with the voltaic battery immersed in water. It is the simplest assumption by means of which the facts can be represented.

On the old theory the assumption of rigid magnetism might be modified to suit the facts by assuming the magnetism to be elastic instead of rigid. Suppose, then, that when the magnet is cut up into spheres, or disks say, the demagnetizing forces of Maxwell (ii. 57) act on the elastic magnetization. There will be a temporary diminution.

It will be understood that I regard these demagnetizing forces as arising out of the fictitious quantities created by analysis for the purpose of compensating the errors of the original hypotheses of Poisson's theory. I therefore prefer the method here indicated. This has the advantage of retaining permanent elements in the permanent magnet ; and it

appears that, so far as the numerical value of the resistance can be obtained, it is at all events not glaringly incompatible with the values of the resistance to external magnetomotive force which have been obtained in an entirely different manner.

XXIX. *On an Arrangement for dividing Inch- and Metre-Scales.*

By R. H. M. BOSANQUET, *St. John's College, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE screw of the slide-rest of the Royal Society's lathe in my laboratory has a pitch of $\frac{1}{8}$ inch. It was therefore an obvious arrangement for dividing decimal-inch scales, to fit this screw with a micrometer-wheel of 25 holes.

20	holes	then	correspond	to	$\frac{1}{10}$	inch.
4	"	"	"	"	$\frac{1}{50}$	inch.
2	"	"	"	"	$\frac{1}{100}$	inch.

The holes are worked with pins, and a V falling over the pins in the same way as in the micrometer of the same lathe described in *Phil. Mag.* x. p. 220.

Before this was completed it occurred to me that a metre-scale could be divided by the same arrangement, if there was any moderate number which would serve as a factor.

The common equivalent is 1 inch = 2.5400 centim. According to Everett's book of units the error of this is about 2 units in the fifth place of decimals; and therefore it is negligible for practical purposes.

$$\begin{aligned} \text{Hence 1 centim.} &= \frac{8}{2.54} \text{ eighths of an inch.} \\ &= \frac{400}{127} \quad " \quad " \end{aligned}$$

It was only necessary therefore to set a division of 127 holes round the micrometer-wheel, and 40 of these holes give a millimetre. If it were desired to divide to tenths of millim., of course 4 of the holes would be used. This division of 127 holes and the one of 25 for the inch-scale are executed on the same wheel, by means of which therefore both metre- and inch-scales can be divided without any shifting whatever.

In dividing millimetres a little piece of brass is used, which reaches from the pin in use to the hole at distance 40, so as to avoid having to count every time.

The division of 127 was executed with the micrometer by means of the following approximation depending on the 67 wheel.

$$\begin{array}{rcl}
 \text{Tangent wheel} & = & 180 \text{ turns,} \\
 \text{Each turn} & = & 67, \\
 \frac{180 \times 67}{127} & = & 95 \text{ less } \frac{5}{127}.
 \end{array}$$

Therefore 95 holes of the 67 wheel are set on at each step, and four times during the process one hole is set back. The resulting error in the division of 127 is quite trifling; and even a considerable error in this respect has but a small influence on the scale.

There is a similar approximation depending on a micrometer-wheel of 60, which any one can make for himself from the ordinary divisions of the lathe-head.

The immediate object was the division of two scales of millimetres and fiftieths of an inch for a bifilar suspension in course of construction.

XXX. *Notices respecting New Books.*

The Elementary Part of a Treatise on the Dynamics of a System of Rigid Bodies, being Part I. of a Treatise on the whole subject; with numerous examples. By EDWARD JOHN ROUTH, F.R.S. Fourth Edition, revised and enlarged. (London: Macmillan, 1882. Pp. xii + 385.)

THE task imposed upon the reviewer of such a work as the one before us is easy. It has reached a fourth edition, and is written by a mathematician than whom no one more competent for this special branch could be found. The success of the work and the careful examination which each successive edition has undergone relieve the reader from the search for errors in statement of principles or in solution of exercises. The outward aspect and the inward arrangement are little different from those of the third edition of 1877. "In order to render the book less bulky for the student, it has been divided into two parts. In the first volume there will be found all the elements of the subject together with some methods which are intended for the more advanced student. In the second part the higher applications will be given." We need say little under the head of arrangement of subjects, which has been carried out on the lines of the last edition; but we notice the use of heavy type at the head of very many of the articles, thus bringing the subject to be treated of more prominently before the student: we notice also in some cases an improved form of arrangement of the matter of the articles. A very cursory comparison of this edition with the last will show that "many additions have been made to every part of the subject."

Chapters I. to VII. treat of the same subjects in the two editions (third and fourth) now before us. In Chapter III. there are four additional articles devoted to the Anemometer called a "Robinson," which consists of four hemispherical cups attached to four horizontal arms which turn round a vertical axis; the articles are founded

upon papers by Dr. Robinson in the 'Irish Transactions' (1850) and by Prof. Stokes in the 'Proceedings of the Royal Society' (1881). In Chapter IV. "Rigidity of Cords" is considered in connexion with Friction. In Chapter V. some articles are introduced upon "Screws" in connexion with the articles upon "Composition of Rotations;" and articles on "Finite Rotations" replace the former articles on "Moving Axes and Relative Motion" and "Motion relative to the Earth:" these new sections contain *Rodrigues' Theorem*, *Sylvester's Theorem*, *Conjugate Rotations*, and *Composition of Screws*. In Chapter VI. there are some additional articles, and a few of the old ones have been removed, but the chief divisions remain unaltered. Chapter VII. has undergone more change than has fallen to the lot of the previous chapters: the articles on "Principles of Least Action and Varying Action" are relegated to the second part, as are those on the "Theory of moving axes and of Motion relative to the Earth" (of old Chapter V.), and Chapters IX. to XII. of the third edition. The *Theory of small oscillations with several degrees of freedom both about a position of equilibrium and a state of steady motion*, and the *Theory of forced oscillations* are also to form part of the advanced course.

Lagrange's Equations, the *Theory of reciprocation* and *Sir W. Hamilton's Equations*, the *modified Lagrangian function*, *non-conservative forces*, and *indeterminate multipliers* form the contents of Chapter VIII. Chapter IX. contains elementary portions of old chapter VIII. on "Small Oscillations;" and Chapter X. is devoted to some special problems on *Oscillations of a rocking body in three dimensions*, *of Cones in three dimensions*, *Large Tautochronous motions*, and *Oscillations of Cylinders and Cones to the Second Order*.

The whole book bears evidence that the author has worked upon this new edition *con amore*, and has done his utmost to bring the treatment as near to perfection as possible. Such a work reflects the highest credit upon the University that has assisted at its birth; and the principle of "thorough" which prevades every part explains how it is that the writer is so successful a teacher.

XXXI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 65.]

January 10, 1883.—J. W. Hulke, Esq., F.R.S.,
President, in the Chair.

THE following communications were read:—

1. "On the Lower-Eocene Section between Reculvers and Herne Bay, and some Modifications in the Classification of the Lower London Tertiaries." By J. S. Gardner, Esq., F.G.S.

The author noticed Prof. Prestwich's classification of the Lower London Tertiaries, and the introduction by the Survey of the term "Oldhaven Beds" for some of his basement beds of the London Clay. He next discussed the conditions under which the Lower Tertiaries were produced, and showed that throughout the Eocenes

there are indications of the close proximity of land and of the access of fresh water. Two types of faunas are to be recognized, namely those of the Calcaire Grossier and the London Clay, the latter indicating more temperate climatal conditions. The former is represented in England by the Bracklesham series. The areas of these two faunas were separated by land forming an isthmus, as each formation is bounded by a shore-line and separated from its neighbours by freshwater formations; but this isthmus probably shifted its position to the north and south without ever being broken through. A vast Eocene river existed, draining a great continent stretching westward; the indications of this river in Hampshire and Dorsetshire would show it to have been there 17 or 18 miles wide.

The Lower Tertiaries have been divided by Prof. Prestwich and the Survey into the marine Thanet Beds, the fluviatile, estuarine, and marine Woolwich and Reading Beds, and the marine Oldhaven Beds. The mode of occurrence of these was described by the author, with especial reference to the section between Herne Bay and the Reculvers, from his investigation of which he was led to the following conclusions:—The Thanet Sands were probably deposited by a rough sea outside the estuary of the great Eocene river, but within its influence. This area became silted up, rose above the surface, and became covered with shingle and sand. The Thanet Beds closed with a period of elevation, during which the Reading Beds were formed; and this was followed by a subsidence during the Woolwich period, which finally ushered in the Oldhaven and London-Clay deposits. The formation of the Oldhaven Beds may be compared with that of the modern beach at Shellness; and during the period of depression the beaches would advance steadily over the flat area of Sheppey, and the earlier formed ones would sink and become covered up by the silt of the great Eocene river. These beaches, forming vast aggregations of sand and shingle between the Thanet Beds and the London Clay, form integral portions of one or other formation, and cannot be recognized as forming a separate formation at all equivalent to the other divisions of the Eocene.

2. "On Mr. Dunn's Notes on the Diamond-fields of South Africa, 1880." By Francis Oats, Esq., F.G.S.

The author referred to the hypothesis put forward in 1880 by Mr. Dunn (*Quart. Journ. Geol. Soc.* vol. xxxvii. p. 609), that the carbon for the production of the South-African diamonds was furnished by the black carbonaceous shales found throughout the district, and the conclusion drawn by him therefrom that therefore diamonds would not be found below the level of these shales. The author stated that the shales, so far as he knows, do not occur below 270 feet, whilst the ground is successfully worked for diamonds at a depth of 350 feet. He maintained that the carbonaceous shales have nothing to do with the origin of the diamonds, and stated that the "craters" containing the diamantiferous rock, at an earlier date erupted quite different material; and he instanced the occurrence in the Kimberley mine of a mass of "dolerite" between the diamantiferous ground and the surrounding shales.

January 24.—Dr. J. Gwyn Jeffreys, F.R.S., Vice-President,
in the Chair.

The following communications were read :—

1. "On *Streptelasma Roëmeri*, sp. nov., from the Wenlock Shale." By Prof. P. Martin Duncan, F.R.S., V.P.G.S.

2. "On *Cyathophyllum Fletcheri*, Edw. & H., sp." By Prof. P. Martin Duncan, F.R.S., V.P.G.S.

3. "On the Fossil Madreporaria of the Great Oolite of the Counties of Gloucester and Oxford." By Robert F. Tomes, Esq., F.G.S.

February 7.—J. W. Hulke, Esq., F.R.S., President,
in the Chair.

The following communications were read :—

1. "On the Metamorphic and Overlying Rocks in parts of Ross and Inverness shires." By Henry Hicks, M.D., F.G.S. With Petrological Notes by Prof. T. G. Bonney, M.A., F.R.S., Sec. G.S.

In this paper the author described numerous sections which have been examined by him in three separate visits made to the north-west Highlands. In some previous papers, sections in the neighbourhood of Loch Maree had been chiefly referred to. Those now described are to the south and south-east of that area, and occur in the neighbourhoods of Achmashellach, Strathcarron, Loch Carron, Loch Trishm, Attadale, Stronoe Ferry, Loch Alsh, and in the more central areas about Loch Shiel and Loch Eil to the Caledonian Canal.

In these examinations the author paid special attention to the stratigraphical evidence, to see whether there were any indications which could in any way be relied upon to prove the theory propounded by Sir R. Murchison that in these areas fossiliferous Lower Silurian rocks dip under thousands of feet of the highly crystalline schists which form the mountains in the more central areas. On careful examination he found that, in consequence of frequent dislocations in the strata, the newer rocks were frequently made to appear to dip under the highly crystalline series to the east, though in reality the appearance in each case was easily seen to be due to accidental causes. Evidences of dislocation along this line were most marked; and the same rocks, in consequence, were seldom found brought together. He recognized in these eastern areas at least two great groups of crystalline schists metamorphosed throughout in all the districts examined, even when regularly bedded and not disturbed or contorted; and they have representatives in the western areas among the Hebridean series, which cannot in any way be differentiated from them. These he called locally by the names, in descending order, of Ben-Fyn and Loch-Shiel series. The former consist, in their upper part, of silvery mica-schists and gneisses, with white felspar and quartz; in their lower part, of hornblendic rocks, with bands of pink felspar and quartz, and of chloritic and epidotic rocks and schists. The Loch-Shiel series consists chiefly of massive granitoid gneisses and hornblendic and black mica-schists. Thirty-three microscopical sections of the

crystalline schists and the overlying rocks are described by Prof. Bonney; and he recognizes amongst them three well-marked types. In No. 1 he includes the Torridon sandstone, the quartzites and the supposed overlying flaggy beds on the east side of Glen Laggan. These are partially metamorphosed; only distinct fragments are always easily recognizable in them in abundance. In No. 2, the Ben-Fyn type, the rocks are crystalline throughout, being typical gneisses and mica-schists. In No. 3, the Loch-Shiel series, he recognizes highly typical granitic gneisses of the Lower Hebridean type. Dr. Hicks failed to find in these areas at any point the actual passage from group 1 to group 2; neither did the same rocks belonging to group 1 meet usually the same rocks belonging to group 2. The evidence everywhere showed clearly that the contacts between these two groups were either produced by faults or by overlapping. Group 3, placed by Murchison as the highest beds in a synclinal trough, supported by the fossiliferous rocks, the author regarded as composed of the oldest rocks in a broken anticlinal. They are the most highly crystalline rocks in these areas; and the beds of group 2 are thrown off on either side in broken folds. These, again, support the rocks belonging to group 1. The author therefore feels perfectly satisfied that the crystalline schists belonging to groups 2 and 3, which compose the mountains in the central areas, do not repose conformably upon the Lower Silurian rocks of the north-west areas with fossils, and that these highly crystalline rocks cannot, therefore, be the metamorphosed equivalents of the comparatively unaltered, yet highly disturbed and crumpled, richly fossiliferous Silurian strata of the southern Highlands, but are, like other truly crystalline schists examined by him in the British Isles, evidently of pre-Cambrian age.

In an Appendix by Prof. T. G. Bonney, F.R.S., Sec.G.S., on the Lithological Characters of a Series of Scotch Rocks collected by Dr. Hicks, the author stated that he observed in the above series, as he had done in other Scotch rocks lately examined by him, three rather well-marked types:—one where, though there is a certain amount of metamorphism among the finer constituents forming the matrix, all the larger grains, quartz, felspar, and perhaps mica, are of clastic origin; a second, while preserving a bedded structure and never likely to be mistaken for an igneous rock, being indubitably of clastic origin, retains no certain trace of original fragments; while the third, the typical “old gneiss” of the Hebridean region, seldom exhibits well-marked foliation. It is sometimes difficult to distinguish between the first and second of these; but this the author believed to be generally due to the extraordinary amount of pressure which some of these Scotch rocks have undergone, which makes it very hard to determine precisely what structures are original. Even the coarse gneiss is sometimes locally crushed into a schistose rock of comparatively modern aspect. The least altered of the above series the author considered to be the true “newer-gneiss” series of the Highlands, but both of the others to be much older than the Torridon Sandstone.

2. "On the Lower Carboniferous Rocks in the Forest of Dean, as represented in typical sections at Drybrook." By E. Wethered, Esq., F.G.S., F.C.S. With an Appendix by Dr. Thomas Wright, F.R.S., F.G.S.

The author described a series of beds overlying the normal Old Red Sandstone and underlying the normal Lower Limestone Shales in the above district. They differ from the ordinary Old Red Sandstone in two particulars:—(1) No fossils characteristic of this series have as yet been discovered in them. (2) The materials of which the Old Red of the neighbourhood is formed are well waterworn, while those composing the beds referred to are not so; they also contain calcareous material; and the author considered them to correspond, in time, with the Calciferous series of Scotland, for the following reasons in addition to their stratigraphical position:—(1) with those of Berwickshire in the rapid succession and variation in the colour of the beds; (2) the presence of certain Polyzoa and of *Rhynchonella pleurodon* in a limestone which succeeds them. The author also described a section in the Millstone Grit at the Morse railway-cutting. Here the Millstone Grit dips at about 40°; resting on it is a rose-coloured sandstone passing up into a pebble-bed dipping at 19°. The pebbles are vein-quartz and a quartzite like that of the Lickey. The Old Red Sandstone, Calciferous Sandstone, and Millstone Grit appeared to him to have derived their materials from a common source, viz. ancient granitic rocks.

An Appendix by Dr. T. Wright described the organisms in specimens of the above-named limestone of Drybrook. Polyzoa are abundant, individuals being numerous, but species few. *Rhabdomeson gracile* and *Fenestella tuberculata* are abundant in one specimen, the other containing in addition *Ceriopora similis*. Fragments of a Crinoid, referred to *Poteriocrinus crassus* also abound. There are a few crushed shells of *Rhynchonella pleurodon*, and spines, possibly of a *Productus*. The organisms of a slab from the Bristol district were also described. This contains *R. gracile* with one or two other Polyzoa, and numerous Crinoidal fragments.

XXXII. Intelligence and Miscellaneous Articles.

PHOSPHOROGRAPHY OF THE INFRA-RED REGION OF THE SOLAR SPECTRUM: WAVE-LENGTH OF THE PRINCIPAL LINES. BY HENRI BECQUEREL.

WHEN, in a dark room, the solar spectrum is projected during a few seconds upon a screen coated with a phosphorescent substance which has previously been exposed to the light, and then the luminous rays are suddenly intercepted, the phosphorescence is observed to have been rendered brighter in the region struck by the violet and ultra-violet radiations, while in the red and infra-red region it has been destroyed; the image of this portion of the spectrum then appears dark on the bright ground of the screen. These phenomena were discovered long ago by my father, and permitted him to determine in the infra-red spectrum the position of several lines and bands analogous to the dark lines of the visible

spectrum*. In repeating and varying these experiments several years since, I was led to various interesting observations, and, in particular, to indicate the position and wave-length of fine lines of which some appear to me to have hitherto escaped the various processes of investigation which have been applied to the study of this region of the spectrum.

It is known that thermoscopic methods reveal the existence in the infra-red spectrum of sundry maxima and minima. In particular MM. Fizeau and Desains in 1847 discovered the existence of a cold band of which the wave-length is 0.001445 millim.; and M. Desains has quite recently† given a description of the calorific spectrum of the sun which extends far beyond the limit observable by means of the phenomena of phosphorescence, and shows, among others, ten bands which appear to answer to those which will be pointed out further on. Mr. Langley‡, with the aid of his bolometer, has likewise given an extensive drawing of the heat-spectrum, which, however, comprises only seven or eight bands in the region that forms the subject of the present memoir.

The red and infra-red rays act upon phosphorescent substances after the manner of heat, by at first accelerating the luminous emissions and causing the substance to give out in a shorter time the same sum of light which it would emit in a longer time and with less intensity if it were sheltered from the radiation or the calorific influence. The experiment above indicated presents two phases: if the spectral influence has been of very short duration, the impressed region appears at first brighter than the ground, and gives a positive image of the spectrum with lines relatively dark; if the impression is prolonged, it exhausts the phosphorescence of the corresponding region, which is extinguished, and the image of the spectrum appears dark with bright lines.

Generally only the second phase of the phenomenon is visible, especially with hexagonal blende, which can give continuously a negative image of the spectrum. Substances of which the phosphorescence lasts a long time, on the contrary, present very clearly the first phase; and the positive image thus obtained exhibits a remarkable fineness of details. I shall not here indicate the simple experimental arrangements which permit these phenomena to be studied with precision; they will be explained in a memoir to be published shortly.

It was very important to vary the nature of the phosphorescent screens. Besides hexagonal blende I used sulphides of strontium and calcium, giving various shades by phosphorescence; and I was led to observe that the phosphorographic images were the superposition of the image of the solar spectrum and maxima and minima of extinction peculiar to each substance, occupying regions in the spectrum variable with each substance. It is moreover easy to distinguish the fine lines of the spectrum from those maxima which represent wide bands where the extinction is more rapid than in the adjacent regions and which are reproduced with various sources of light.

* *Comptes Rendus*, t. lxxvii. p. 302 (1873), & t. lxxxiii. p. 249 (1876).

† *Comptes Rendus*, t. xcv. p. 434 (1882). ‡ *Ibid.* p. 482 (1882).

I proposed to determine the wave-lengths of the lines revealed by the phenomena of phosphorescence by projecting, on the substances above referred to, the spectra furnished by a reticulation. I am indebted to the kindness of M. Mascart for the use of a fine reticulation on metal by Mr. Rutherford, as well as for another traced on glass. The first inconvenience met with is that the infra-red region of the first spectrum is entirely superposed to the ultra-violet and luminous region of the second spectrum. These rays are eliminated by the interposition of red glass, which does not arrest the infra-red radiations studied.

When it was possible to bring to the point the lines of both spectra at the same time, I took advantage of their coincidence to deduce the infra-red wave-lengths by doubling the known wave-lengths of the Fraunhofer lines. Thus were determined, with the small reticulation, by transmission, the wave-lengths of two lines:—one 0·000976 millim., near 2F; the other 0·001098, between 2b and 2D.

With the reflecting reticulation the simultaneous bringing to the point the two spectra was not possible; and the position of the infra-red lines was simply taken from the phosphorescent screen by intercepting the more refrangible part of the second spectrum. The corresponding deviations, and consequently the wave-lengths, were determined by a trigonometrical calculation, the elements of which could be directly measured, and were moreover deduced very exactly from the data corresponding to three lines of known wave-length—for example, A, B, and C. By reason of the weakening of the luminous intensity, the measurements with the reflecting reticulation could not be extended beyond the wave-length 0·000918. In short the following numbers were obtained:—

Lines.		Wave-lengths.	Remarks.
		millim.	
Group A.	A	0·0007604	(Fraunhofer.)
	A ₁ *	0·0007819	
	A ₂	0·0007957	
	A ₃ *	0·0008110	Strong extinction-band.
	A ₄ *	0·0008360	
Group A'.	A ₀	0·0008630	Luminous region.
	A ₁ *	0·0008850	
	A ₂	0·0008980	Strong extinction-band.
	A ₃ *	0·0009180	
	A ₄	0·0009490	(Interpolation.)
Group A''.	A ₅ *	0·0009760	
	A ₆	0·0010060	(Interpolation.)
	A ₀	0·0010500	(Interpolation.)
	A ₁ * ..	0·0010980	
	A ₀	0·0011760	(Interpolation.)
Group A'''.	A ₁ * ..	0·0012200†	
	A ₂ * ..	0·0013120	(Extrapolation.)
	A _{iv}	0·0014440	

[† A strong extinction-band for blende; edge very distinct on the less-refrangible side.—ED. BECQUEREL.]

The lines marked with an asterisk were observed in 1876 by my father, who determined the wave-length 0.001220 by observation of the interference-bands which are produced in the spectrum by previous reflection of the luminous rays upon a thin film of air.

The last two numbers of the preceding Table are obtained by carefully tracing a curve giving the wave-lengths for the various positions in the spectrum and prolonging it beyond the line A_1''' , which answers to the last number found by experiment; and of those two numbers the last presents the curious coincidence that it is very nearly the wave-length of the cold band determined by M. Fizeau.

The new points resulting from the present researches are, besides the determination of new lines of the solar spectrum and their wave-lengths, the observation in the infra-red portion of maxima and minima of extinction proper to various phosphorescent substances, manifested by various luminous sources and analogous to the phosphorogenic maxima and minima of the other end of the spectrum.—*Comptes Rendus de l'Académie des Sciences*, Jan. 8, 1883, t. xvi. pp. 121–124.

ON THE MEASUREMENT OF THE PHOTOMETRIC INTENSITY OF THE SPECTRAL LINES OF HYDROGEN. BY H. LAGARDE.

The spectrum of a gas, under determined conditions of temperature and pressure, is not completely defined by the wave-lengths of the various lines which compose it. When the pressure and the calorific energy of the discharge are varied, the intensity of those lines is modified according to an unknown law; it may even become *nil* for one or more particular lines, under certain conditions; while other lines may become visible for particular values of the temperature and pressure. These variations of intensity therefore, in each circumstance of the experiment, change the physiognomy of the spectrum, which will not be defined unless the intensities of the lines composing it be given. It is to the study of the measurement of these intensities that I have applied myself.

In absolute value, the radiant energy of a vibration of a determined wave-length ought to be expressed in thermal or mechanical units; but the feebleness of gas-spectra forbids any direct attempt in that way, and necessitates the use of a photometric comparison.

The employment of a spectrophotometer disposed so as to give precise and comparable valuations is of course imposed, as M. Crova has shown*, in determinations of this sort. The capillary portion of the spectrum-tube being placed opposite half the slit of the instrument and at a constant distance, the light from a lamp, having traversed a system of two nicols, one movable upon a graduated circle, is received laterally upon a prism with double total reflection, which covers the other half of the slit. If the slit be opened a little, the lines of the spectrum take sufficient breadth to fill the ocular slit and are in immediate contact with the portion of the spectrum of the lamp possessing the same wave-length; the rotation of the nicol is measured which gives equality of intensity.

* *Comptes Rendus*, 1881, t. xciii. p. 512; *Ann. de Chim. et de Phys.* [5] xxii. p. 513.

With a little practice this measurement is susceptible of great precision if a good spectrophotometer is employed. The new instrument which I use, constructed by M. Duboscq according to the directions of M. Crova, leaves nothing to be desired in this respect.

The lamp employed is the Carcel standard, placed on the automatic balance of M. Deleuil, the course and regulation of which have been previously investigated*. The intensities obtained are reduced to a normal consumption of 42 grammes of oil per hour.

My researches have been directed to the spectrum of hydrogen, which has the advantage of giving very pure lines situated in different regions of the spectrum. The spectrum-tube, with aluminium electrodes, was illuminated at the commencement of the researches by the spark of an ordinary Ruhmkorff coil actuated by three Bunsen elements, and later by a Holtz machine. To effect the variation of the calorific energy of the spark, I confined myself at first to interposing in the inducing circuit variable lengths ($R=14, 10, 6, 2$ centim.) of fine German-silver wire. This first disposition permits the direction of the phenomenon to be distinctly seen; it has been replaced (for some definitive measurements in course of execution) by another, more precise.

The gas-tube can be put into communication, by a three-way cock, either with a system of tubes containing phosphoric anhydride communicating with a pure-hydrogen generator or with the apparatus for producing the vacuum. The initial step is the production of the vacuum by means of Alvergnyat's mercury pump; it is completed by means of a mercury trompe with six falls, constructed by MM. Alvergnyat frères. With the aid of this fine apparatus all the pressures comprised between 7 millim. (at which the spark begins to pass) and 0.000001 of an atmosphere are obtained. MacLeod's gauge, attached to the instrument, accurately measures those pressures.

Here are some of the series obtained for the lines $H\alpha$, $H\beta$, $H\gamma$, on varying the pressure and the energy of the discharge (the intensity of the corresponding regions of the lamp-spectrum, with the nicols parallel, being arbitrarily put = 1000):—

		Lines.		
		Red.	Blue.	Violet.
Pressure 6.5 millim.				
R	14	3.6	5.5	17.2
R	10	6.2	7.5	18.1
R	6	7.5	12.4	19.6
R	2	9.5	22.6	36.7
		Lines.		
		Red.	Blue.	Violet.
Pressure 0.542 millim.				
R	14	8.8	25.8	65.8
R	10	12.9	34.2	86.8
R	6	28.3	72.5	140.2
R	2	49.4	152.1	240.9

* "Détermination du pouvoir éclairant des radiations simples," par MM. Crova et Lagarde, *Journal de Physique*, [2] i. 1882.

			Lines.		
		Pressure	Red.	Blue.	Violet.
		0·010 millim.			
R	14	12·6	39·3	110·9
R	10	17·8	55·0	133·8
R	6	38·5	94·9	176·4
R	2	76·1	183·2	289·6

The curves traced by means of these values show the inequality of intensity of the three lines, an inequality variable with the induced discharge. In proportion as the pressure diminishes, the ordinates are augmented considerably, and the curve rises throughout. For the pressure of 6·5 millim. the curve of the red line becomes a straight line.

In the various conditions in which I have operated, I have been able to make a pretty large number of observations on the influence of impurities, the nature of the stratifications, and the production of the sensitive state* in rarefied tubes. I am continuing these experiments, measuring the difference of potentials at the two extremities of the tube and illuminating the tube by the discharge of the Holtz machine.

It would be premature to draw any definitive conclusions from the present results; I wished, in this first note, to confine myself to indicating the first results of the investigation and showing the general course of the phenomenon.—*Comptes Rendus de l'Académie des Sciences*, Dec. 26, 1882, t. xev. pp. 1350–1352.

CENTRAL FORCES AND THE CONSERVATION OF ENERGY.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

If your correspondent, Mr. von Tunzelmann, will refer again to my paper, he will see that his criticism in your February number is not justified. I had at the outset (p. 35) assumed F to be the component of the moving force in the line joining the two particles considered; and it was subsequently shown (p. 38) that if there were any component at right angles to that line, the conservation of energy would not hold. The argument on p. 37 is therefore perfectly legitimate.

WALTER R. BROWNE.

ERRATUM.

In the footnote to Prof. Silvanus Thompson's paper in our last Number, p. 124, it was stated by inadvertence that it was communicated by the Physical Society. It should have read *communicated by the Author*; the paper, however, was *subsequently* communicated to the Physical Society.

* W. Spottiswoode and Moulton, *Philosophical Transactions*, 1879, p. 165.

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[FIFTH SERIES.]

APRIL 1883.

XXXIII. *On Maintained Vibrations.* By Lord RAYLEIGH,
*F.R.S., Professor of Experimental Physics in the University
of Cambridge*.*

WHEN a vibrating system is subject to dissipative forces, the vibrations cannot be permanent, since they are dependent upon an initial store of energy which suffers gradual exhaustion. In the usual equation

$$\frac{d^2\theta}{dt^2} + \kappa \frac{d\theta}{dt} + n^2\theta = 0 \quad (1)$$

κ is positive, and the solution indicates the progressive decay of the vibrations in accordance with the exponential law. In order that the vibrations may be maintained, the vibrating body must be in connexion with a source of energy. This condition being satisfied, two principal classes of maintained vibrations may be distinguished. In the first class the magnitude of the force acting upon the body in virtue of its connexion with the source of energy is proportional to the amplitude, and its phase depends in an approximately constant manner upon the phase of the vibration itself; in the second class the body is subject to influences whose phase is independently determined.

The first class is by far the more extensive, and includes

* Communicated by the Author.

vibrations maintained by wind (organ-pipes, harmonium-reeds, æolian harps, &c.), by heat (singing flames, Rijke's tubes, &c.), by friction (violin-strings, finger-glasses, &c.), as well as the slower vibrations of clock-pendulums and of electromagnetic tuning-forks. When the amplitude is small, the force acting upon the body may be divided into two parts, one proportional to the displacement θ (or to the acceleration), the second proportional to the velocity $d\theta/dt$. The inclusion of these forces does not alter the *form* of (1). By the first part (proportional to θ) the pitch is modified, and by the second the coefficient of decay*. If the altered κ be still positive, vibrations gradually die down; but if the effect of the included forces be to render the complete value of κ negative, vibrations tend on the contrary to increase. The only case in which according to (1) a steady vibration is possible, is when the complete value of κ is zero. If this condition be satisfied, a vibration of any amplitude is permanently maintained.

When κ is negative, so that small vibrations tend to increase, a point is of course soon reached after which the approximate equations cease to be applicable. We may form an idea of the state of things which then arises by adding to equation (1) a term proportional to a higher power of the velocity. Let us take

$$\frac{d^2\theta}{dt^2} + \kappa \frac{d\theta}{dt} + \kappa' \left(\frac{d\theta}{dt} \right)^3 + n^2\theta = 0, \quad \dots \quad (2)$$

in which κ and κ' are supposed to be small. The approximate solution of (2) is

$$\theta = A \sin nt + \frac{\kappa'nA^3}{32} \cos 3nt, \quad \dots \quad (3)$$

in which A is given by

$$\kappa + \frac{3}{4}\kappa'n^2A^2 = 0. \quad \dots \quad (4)$$

From (4) we see that no steady vibration is possible unless κ and κ' have different signs. If κ and κ' be both positive, the vibration in all cases dies down; while if κ and κ' be both negative, the vibration (according to (2)) increases without limit. If κ be negative and κ' positive, the vibration becomes steady and assumes the amplitude determined by (4). A smaller vibration increases up to this point, and a larger vibration falls down to it. If, on the other hand, κ be positive, while κ' is negative, the steady vibration abstractedly possible is

* For more detailed application of this principle to certain cases of maintained vibrations, see Proceedings of the Royal Institution, March 15, 1878.

unstable, a departure in either direction from the amplitude given by (4) tending always to increase.

Of the second class the vibrations commonly known as *forced* have the first claim upon our attention. The theory of these vibrations has long been well understood, and depends upon the solution of the differential equation formed by writing as the right-hand member of (1) $P \cos pt$ in place of zero. The period of steady vibration is coincident with that of the force, and independent of the natural period of vibration; but the amplitude of vibration is greatly increased by a near agreement between the two periods. In all cases the amplitude is definite and is proportional to the magnitude of the impressed force. When the force, though strictly periodic, is not of the simple harmonic type, vibrations may be maintained by its operation whose period is a submultiple of the principal period.

There is also another kind of maintained vibration which from one point of view may be considered to be forced, inasmuch as the period is imposed from without, but which differs from the kind just referred to in that the imposed periodic variations do not tend directly to displace the body from its configuration of equilibrium. Probably the best-known example of this kind of action is that form of Melde's experiment in which a fine string is maintained in transverse vibration by connecting one of its extremities with the vibrating prong of a massive tuning-fork, *the direction of motion of the point of attachment being parallel to the length of the string**. The effect of the motion is to render the tension of the string periodically variable; and at first sight there is nothing to cause the string to depart from its equilibrium condition of straightness. It is known, however, that under these circumstances the equilibrium position may become unstable, and that the string may settle down into a state of permanent and vigorous vibration, *whose period is the double of that of the point of attachment*†.

The theory of vibrations of this kind presents some points of difficulty, and does not appear to have been treated hitherto. In the present investigation we shall start from the assumption that a steady vibration is in progress, and inquire under what circumstances the assumed state of things is possible.

If the force of restitution, or "spring," of a body susceptible of vibration be subject to an imposed periodic variation,

* When the direction of motion is transverse, the case falls under the head of ordinary forced vibrations.

† See Tyndall's 'Sound,' 3rd ed. ch. iii. § 7, where will also be found a general explanation of the mode of action.

the differential equation becomes

$$\frac{d^2\theta}{dt^2} + \kappa \frac{d\theta}{dt} + (n^2 - 2\alpha \sin 2pt)\theta = 0, \quad . \quad . \quad . \quad (5)$$

in which κ and α are supposed to be small. A similar equation would apply approximately in the case of a periodic variation in the effective mass of the body. The motion expressed by the solution of (5) can only be regular when it keeps perfect time with the imposed variations. It will appear that the necessary conditions cannot be satisfied rigorously by any simple harmonic vibration; but we may assume

$$\theta = A_1 \sin pt + B_1 \cos pt + A_3 \sin 3pt + B_3 \cos 3pt \\ + A_5 \sin 5pt + \dots, \quad . \quad . \quad . \quad (6)$$

in which it is not necessary to provide for sines and cosines of even multiples of pt . If the assumption is justifiable, the series in (6) must be convergent. Substituting in the differential equation, and equating to zero the coefficients of $\sin pt$, $\cos pt$, &c., we find

$$\begin{aligned} A_1(n^2 - p^2) - \kappa p B_1 - \alpha B_1 - \alpha B_3 &= 0, \\ B_1(n^2 - p^2) + \kappa p A_1 - \alpha A_1 - \alpha A_3 &= 0, \\ A_3(n^2 - 9p^2) - 3\kappa p B_3 - \alpha B_1 + \alpha B_5 &= 0, \\ B_3(n^2 - 9p^2) + 3\kappa p A_3 + \alpha A_1 - \alpha B_5 &= 0, \\ A_5(n^2 - 25p^2) - 5\kappa p B_5 - \alpha B_3 + \alpha B_7 &= 0, \\ B_5(n^2 - 25p^2) + 5\kappa p A_5 + \alpha A_3 - \alpha A_7 &= 0, \\ . & . & . & . & . & . & . \end{aligned}$$

These equations show that relatively to A_1, B_1, A_3, B_3 are of the order α ; that relatively to A_3, B_3, A_5, B_5 are of the order α , and so on. If we omit A_3, B_3 in the first pair of equations, we find as a first approximation,

$$\begin{aligned} A_1(n^2 - p^2) - (\kappa p + \alpha)B_1 &= 0, \\ A_1(\kappa p - \alpha) + (n^2 - p^2)B_1 &= 0; \end{aligned}$$

whence

$$\frac{B_1}{A_1} = \frac{n^2 - p^2}{\kappa p + \alpha} = \frac{\alpha - \kappa p}{n^2 - p^2} = \frac{\sqrt{(\alpha - \kappa p)}}{\sqrt{(\alpha + \kappa p)}}, \quad . \quad . \quad . \quad (7)$$

and

$$(n^2 - p^2)^2 = \alpha^2 - \kappa^2 p^2. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Thus, if α be given, the value of p necessary for a regular motion is definite; and p having this value, the regular motion is

$$\theta = P \sin (pt + \epsilon),$$

in which ϵ , being equal to $\tan^{-1}(B_1/A_1)$, is also definite. On the other hand, as is evident at once from the linearity of the original equation, there is nothing to limit the amplitude of vibration.

These characteristics are preserved however far it may be necessary to pursue the approximation. If A_{2m+1} , B_{2m+1} , may be neglected, the first m pairs of equations determine the *ratios* of all the coefficients, leaving the absolute magnitude open; and they provide further an equation connecting p and α , by which the pitch is determined.

For the second approximation the second pair of equations gives

$$A_3 = \frac{\alpha B_1}{n^2 - 9p^2}, \quad B_3 = -\frac{\alpha A_1}{n^2 - 9p^2},$$

whence

$$\theta = P \sin(pt + \epsilon) + \frac{\alpha P}{9p^2 - n^2} \cos(3pt + \epsilon); \quad (9)$$

and from the first pair

$$\tan \epsilon = \left\{ n^2 - p^2 + \frac{\alpha^2}{n^2 - 9p^2} \right\} \div (\alpha + \kappa p), \quad (10)$$

while p is determined by

$$(n^2 - p^2)^2 - \frac{\alpha^4}{(n^2 - 9p^2)^2} = \alpha^2 - \kappa^2 p^2. \quad (11)$$

Returning to the first approximation, we see from (8) that the solution is only possible under the condition that $\alpha > \kappa p$. If $\alpha = \kappa p$, then $p = n$; i.e. the imposed variation in the "spring" must be exactly twice as quick as the natural vibration of the body would be in the absence of friction. From (7) it appears that in this case $\epsilon = 0$, which indicates that the spring is a minimum one eighth of a period *after* the body has passed its position of equilibrium, and a maximum one eighth of a period *before* such passage. Under these circumstances the greatest possible amount of energy is communicated to the system; and in the case contemplated it is just sufficient to balance the loss by dissipation, the adjustment being evidently independent of the amplitude.

If $\alpha < \kappa p$, sufficient energy cannot pass to maintain the motion, whatever may be the phase-relation; but if $\alpha > \kappa p$, the equality between energy supplied and energy dissipated may be attained by such an alteration of phase as shall diminish the former quantity to the required amount. The alteration of phase may for this purpose be indifferently in either direction; but if ϵ be positive, we must have

$$p^2 = n^2 - \sqrt{\{\alpha^2 - \kappa^2 p^2\}};$$

while if ϵ be negative,

$$p^2 = n^2 + \sqrt{\alpha^2 - \kappa^2 p^2}.$$

If α be very much greater than κp , $\epsilon = \pm \frac{1}{4}\pi$, which indicates that when the system passes through its position of equilibrium the spring is at its maximum or at its minimum.

The inference from the equations that the adjustment of pitch must be absolutely rigorous for steady vibration will be subject to some modification in practice; otherwise the experiment could not succeed. In most cases n^2 is to a certain extent a function of amplitude; so that if n^2 have very nearly the required value, complete coincidence is attainable, without other alteration in the conditions of the system, by the assumption of an amplitude of large and determinate amount.

When a particular solution of (5) has been found, it may be generalized by a known method. Thus, if $\theta = A\theta_1$, we have as the complete solution

$$\theta = A\theta_1 + B\theta_1 \int_0^t \theta_1^{-2} e^{-\kappa t} dt.$$

which may be put into the form

$$\theta = P\theta_1 - B\theta_1 \int_t^\infty \theta_1^{-2} e^{-\kappa t} dt. \quad . \quad . \quad (12)$$

When t is great, the second term diminishes rapidly, and the solution tends to assume the original form $\theta = P\theta_1$.

The number of cases falling under the present head which have been discovered and examined hitherto is not great. The mysterious *son rauque* of Savart, which sometimes accompanies the longitudinal vibrations of bars, and is attributed by Terquem to an associated transverse vibration, is doubtless of this character. Just as in Melde's experiment already spoken of, the periodic variations of tension accompanying the longitudinal vibrations will throw the bar into lateral vibration, if there happen to be a mode of such vibration whose pitch is nearly enough coincident with the *suboctave* of the principal note.

For a lecture illustration we may take a pendulum formed of a bar of soft iron and vibrating on knife-edges. Underneath the pendulum is placed symmetrically a vertical bar electromagnet, through which is caused to pass an electric current rendered intermittent by an interrupter whose frequency is twice that of the pendulum. The magnetic force does not tend to displace the pendulum from its equilibrium position, but produces the same sort of effect as if gravity were subject to a periodic variation.

A similar result is obtained by causing the point of support of the pendulum to vibrate in a *vertical* path. If we denote this motion by $\eta = \beta \sin 2pt$, the effect is as if gravity were variable by the term $4p^2\beta \sin 2pt$. Of the same nature are the crispations observed by Faraday and others on the surface of water which oscillates vertically. Faraday arrived experimentally at the conclusion that there were two complete vibrations of the support for each complete vibration of the liquid. This view has been contested by Matthiessen*, who maintains that the vibrations are isoperiodic. By observations, which I hope to find another opportunity of detailing, I have convinced myself that in this matter Faraday was perfectly correct. The vibrations of water standing upon a horizontal glass plate, which was attached to the centre of a vibrating iron bar, were at the rate of 15 per second when the vibrations of the bar were at the rate of 30 per second. The only difference of importance between this case and that of the pendulum is that, whatever may be the rate of vibration of the plate, there is always possible a free water-vibration of nearly the same frequency, and that consequently no special tuning is called for.

XXXIV. *On a Measurement of Wave-lengths in the Ultra-red Region of the Spectrum of the Sun.* By ERNST PRINGSHEIM†.

IN order to investigate the solar spectrum it is of special importance to know the wave-length of the extreme rays emitted by the sun, and thus to determine the extent of the entire spectrum. In order to determine the wave-length of the least-refrangible rays, Müller‡, and subsequently Lamansky§, observed with the aid of a thermopile the index of refraction of the extreme rays of a spectrum projected by a crown-glass or rock-salt prism, and from that index calculated the wave-length by means of an empirical formula, the correctness of which was controllable only within the limits of the visible rays. The untrustworthiness of this method is obvious; thus, from the same observation Müller calculated, by two different formulæ, for the extreme wave-length the values 0.00177 and 0.0048 millim.

A trustworthy determination of the wave-length is possible only with the aid of the interference of the rays; and this course was taken by Abney||, who succeeded in photographing

* Pogg. *Ann.* cxli. 1870.

† Translated from Wiedemann's *Annalen*, 1883, No. 1, pp. 32-45.

‡ Pogg. *Ann.* cv. p. 352 (1858).

§ *Ibid.* cxlvi. 1872.

|| Phil. Trans. 1880, pp. 653-667.

the ultra-red portion of a diffraction-spectrum. As therein the extreme rays of the spectrum of the first order coincided with the light-rays of the spectrum of the second order, he could easily infer from the known wave-length of these light-rays that of the extreme heat-rays. He found for it the number 0.001075 millim.

Yet we have no right to regard the rays thus defined as really the extreme heat-rays coming from the sun to the earth; the number thus found has only an individual significance relative to the particular experiment made, and teaches us nothing further about the sun's radiation than that rays having a wave-length of 0.001075 millim. are found therein. Hence the investigation is by no means concluded, and it is desirable to identify rays of still greater wave-length in the solar spectrum by fresh observations. Such an investigation I carried out in the course of the summer of 1882, in the Physical Institute of the Berlin University.

I. Apparatus.

As the thing required is to verify the presence of rays whose intensity appears to be very slight, the success of the investigation depends essentially on two main conditions. Namely, first, care must be taken that the intensity of the spectrum observed is as considerable as possible; and, secondly, an instrument must be employed for the demonstration of the extreme rays sought which possesses the greatest possible sensitiveness for the class of rays in question.

To produce a spectrum of the utmost intensity, I employed an excellent plane metallic reflecting diffraction-grating, prepared by Chapman with Rutherford's machine, possessing a square aperture of 43.3 millim. side, and having, according to the maker's statement, 17,296 lines to the inch. This gives for the distance d between each two lines of the grating 0.00146852 millim.

In order to control this datum, observations of the lines D and F were undertaken with the grating; and from the known wave-lengths of these lines the mean value of d was found = 0.0014849 millim.; so that we can pretty safely put

$$d = 0.001485 \text{ millim.}$$

In order to utilize the greatest possible number of rays for the production of the spectrum, all the sun's rays reflected from the metallic mirror of a Duboscq heliostat were concentrated, by means of a circular silver mirror of 90 millim. diameter and about 200 millim. focal distance, upon the square aperture of a slit, of 1.3 millim. side, placed in the focus of

the mirror. This slit served as the luminous object; and an image of it was projected by a second silver mirror of the same dimensions, at the distance of about 1700 millim. In the path of the rays proceeding from this second mirror the diffraction-grating was inserted so that the cone of light exactly covered its entire aperture. Hence the concave mirror did not project a mere image of the slit, but the grating produced on both sides of the image a series of diffraction-spectra. One of these spectra, of the first order, was used for the measurement of the wave-lengths.

The silver mirrors above mentioned were made by silvering and polishing the concave side of plano-concave glasses; the slit was formed by two strips of sheet metal 2 millim. broad connected by cross strips.

In order to render the extreme ultra-red rays perceptible I did not, like Abney, employ their chemical effect, but their thermal action was made visible by a radiometric torsion-apparatus constructed by me. The arrangement of this apparatus I have described in a previous paper*; here let the statement suffice, that the rays falling on the lampblack side of a thin mica plate suspended in a vacuum produced a motion of this disk. A small mirror joined to the disk rotated simultaneously with it, by which a reflection of light was thrown upon a scale. The motion of this reflection served as an indication and a measure of the deflection of the vane. The apparatus was connected with a mercury air-pump; yet it would have sufficed for the present experiment if it had been made in a somewhat simplified form so that it could have been hermetically closed after one exhaustion.

The course of the rays in the experiment is clearly shown by the diagram, fig. 1.

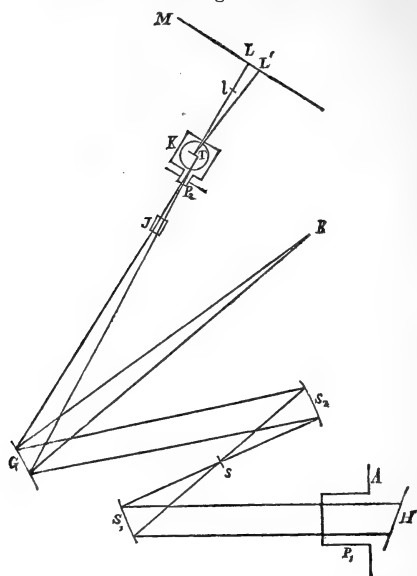
The rays emanating from the heliostat H, set up outside the window, pass first through an aperture A in the closed shutter into the almost completely darkened room, then through a pasteboard tube P_1 about 500 millim. long, directly attached to the shutter, and fall upon the first silver mirror S_1 at a distance of 2510 millim. from the heliostat. By this mirror they are concentrated upon the slit s , then arrive at the second mirror S_2 , pass from this to the grating G, and are here in part directly reflected so as to give rise to an image in B of the slit s , in part dispersed. The extreme ultra-red rays of the spectrum of the first order can be collected in T, where they are incident upon the mica plate.

For the sake of clearness, the angles of incidence of the rays upon the mirrors S_1 and S_2 are drawn much too large in

* Phil. Mag. February 1883, p. 101.

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the figure; in reality the rays were caused to pass as near

Fig. 1.



one another as the breadth of the strips forming the slit s permitted.

The torsion-apparatus, in order to protect it from all lateral radiations and from air-currents, was covered with a tin case filled with wadding, in which was an aperture opposite the mica plate to admit the rays. In front of this aperture a pasteboard tube P_2 , passing through a vertical pasteboard disk, was fixed. Opposite to the mirror of the torsion-apparatus, at a distance of about 1040 millim., was the millimetre-scale M , upon which the lamp L , with the aid of the lens l and a second aperture in the case K , produced the light-reflection L' .

As the action of the rays upon our apparatus extended far beyond the dark interval between the spectra of the first and second orders, and consequently the action of the luminous rays of the second spectrum combined with that of the heat-rays of the first, these two locally coincident spectra had to be separated. For this purpose, at first a solution of iodine in sulphide of carbon was used, which was inserted at J in the path of the rays, and absorbed all the visible ones, while it transmitted the greatest part of the heat-rays. The solution was contained in a glass tube having its two extremities

ground smooth and closed with plates of rock-salt of 2 millim. thickness. It being very inconvenient and difficult to keep the faces of the rock-salt clean and clear for any length of time, this way of filtering the rays is troublesome and expensive. Subsequently, therefore, instead of the iodine solution, I used a perfectly black plate of ebonite of 0.3 millim. thickness, this substance being, according to Abney and Festing*, very diathermanous for rays of great wave-length. This plate showed itself in fact very transmissive also for the extreme ultra-red rays; and its employment is very convenient.

In our arrangement all the reflections took place at metallic surfaces; and any passing of the rays through glass was completely avoided till they reached the very thin glass bulb of the torsion-apparatus, through which the rays had to pass before meeting the radiometer-vane. As, however, any absorption of rays by this glass acts upon the vane by producing motion in the same direction as the absorption by the lamp-black†, even the rays which may have been absorbed by the glass bulb were also effective.

Although the superposition of the luminous rays of the second and the obscure rays of the first spectrum affords a convenient means for approximately estimating the wave-length of the obscure rays occurring in each place, I arranged the setting-up of the apparatus so that an exact measurement of the wave-length was possible. For this purpose I placed the diffraction-grating G on a spectrometer-stand, movable round a vertical axis, from which the telescopes were removed. The position of the stand could be read on the orientation-circle, which was divided into intervals of five minutes of arc, by means of a microscope fixed by an arm to the stand, and the thread cross of which could be displaced laterally with the aid of a micrometer-screw, so as to permit whole minutes to be read off directly. The spectrometer had three adjusting-screws in its base; further, the stand stood separately upon three screws; and, lastly, the mirror G was placed in a box which, again, rested on three adjusting-screws; so that the stand could be shifted at discretion with respect to the micrometer, and the level of the mirror with respect to that of the stand.

II. *Method.*

As in our arrangement the grating only can be rotated, while all the other parts of the apparatus are fixed, it was necessary to employ a somewhat different way of measuring

* Abney and Festing, *Chemical News*, xliii. pp. 176, 177 (1881).

† *Conf.* Pringsheim, *Phil. Mag.* February 1883, pp. 105-110.

from that employed in determining wave-lengths in the visible spectrum, in which the grating is generally fixed, and the observing telescope can be rotated.

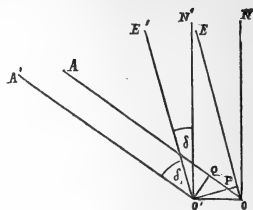
Let E and E' (fig. 2) be two rays, supposed parallel, incident at O and O' upon two neighbouring lines of the grating, and A and A' the two rays, likewise supposed parallel, which remain at the common interference in T (fig. 1), and whose wave-length is to be measured. Let N and N' be the normals erected at O and O' upon the plane of the grating; then we had, with our arrangement, the case (likewise taken into account in the drawing) that the incident and the observed ray are on the same side of the normal to the grating. If we draw O'P perpendicular to E, O'Q perpendicular to A, and put $OO' = d$, the angles $NOE = N'O'E' = \delta$, and the angles $AOE = A'O'E' = \delta_1$, then the difference of path of the rays A and A' when they arrive at the interference, and consequently the wave-length λ of the rays there remaining of the spectrum of the first order, is

$$\begin{aligned}\lambda &= PO + OQ \\ &= d [\sin (PO'O) + \sin (QO'O)] \\ &= d [\sin \delta + \sin (\delta + \delta_1)].\end{aligned}$$

The complete theory of interference teaches that this formula still holds good when, instead of the two rays E and E', a great number of rays are present which are incident upon several apertures of the width d . Since the magnitude of d is known, the measuring of the angles δ and δ_1 suffices for the determination of λ .

In order to find the incidence-angle δ , first the stand with the grating was placed so that the normal to the grating coincided with the incident ray; and then the number on the scale at which the thread cross of the microscope stood, in this position, was noted. This number we will call a . The grating was then placed so that the direct image of the slit just reached our torsion-apparatus; let the corresponding position of the thread cross of the microscope be called b . Lastly, by further turning the stand, that orientation was given to the mirror in which the torsion-apparatus was just hit by the extreme rays operative upon it. With this position the thread cross is at the number c ; and since it forms a constant angle with the normal to the grating, we can also refer

Fig. 2.



the numbers a , b , and c directly to the position of that normal in the three different orientations. The delineation (fig. 2) refers to the third position, in which the normal to the grating is defined by the number c , while to the incident ray E, which in the first position coincided with the normal, the number a still belongs. Hence the angle of incidence is

$$\delta = a - c,$$

if, as was the case with our arrangement, the mirror has to be rotated from the first position to the third in the direction of diminishing numbers.

Since in the second position the direct reflection has the same orientation as the ray A in the figure, the angle δ_1 between A and E is equal to the angle between the direct reflection in the second position and the incident ray E. Now the angle between the mirror-normal in the second position and the ray E is equal to $b - a$, since the mirror must be turned from the first to the second orientation in the direction in which the numbers increase; therefore the angle δ_1 between the direct reflection and E is

$$\delta_1 = 2(b - a).$$

From the observation of the quantities a , b , and c we can therefore calculate δ and δ_1 , and consequently λ also.

III. *Measuring.*

In order that the numbers a , b , c , read off from the orientation-circle may actually be connected with the angles δ and δ_1 in the way above supposed, it is necessary that the plane of incidence of the rays remain the same at all positions of the grating, and parallel to the plane of the orientation-circle. To effect this, first the spectrometer was set up, with the aid of the adjusting-screws attached to its feet, so that the plane of the orientation-circle was horizontal, which was rendered visible by a spirit-level placed on it. Then the stand was placed, by means of its special screws, so that its orientation was horizontal, and kept that position while being rotated 360°. Thereupon the grating was mounted on the stand, and at first in the position in which the incident rays were perpendicular to the plane of the grating.

In order to be able to effect this arrangement with greater exactness, instead of the pasteboard tube P_1 (fig. 1) a pasteboard disk with a rectangular aperture of 16 millim. height and 3 millim. breadth was put before the aperture A in the window-shutter. By this the incident pencil was made much narrower, and hence a more accurate adjustment was rendered possible. Moreover, in the slit s a cross of fine wire was

fixed, and the grating placed so that the image of the cross reflected from the mirror G to the mirror S_2 , passing through the slit s fell upon the mirror S_1 , and, reflected by this, appeared on the screen in A somewhat enlarged. The apparatus was then so arranged that the deviation in the horizontal plane was corrected by rotating the stand on which the grating stood, while half of the deviation in the vertical was obviated by one of the adjusting-screws attached to the base of the grating, the other half by varying the altitude of the mirror S_2 . In this manner the setting-up was regulated so that the centre of the wire cross came to lie exactly in the centre of the rectangular aperture. In order to maintain the plane of incidence invariable with any other position of the grating, the position of the horizontal line proceeding from the horizontal wire of the cross, which ran through all the spectra, was fixed by a lead-pencil line on the pasteboard disk in front of the torsion-apparatus; and then the two other screws in the stand of the grating-mirror were adjusted so that the horizontal line always kept the same position with any rotation of the micrometer-stand. After this the mirror was turned back again into its previous orientation; and the same manipulation continued till the grating had the correct adjustment in all orientations.

In the second position also, in which the directly reflected ray fell on the torsion-apparatus, the entire aperture A was not employed, but only the small cut-out aperture, in order to make a more accurate adjustment possible. In doing this it was of course requisite to take care that the image of the aperture fell exactly on the centre of each of the mirrors.

In order that in this position also the directly reflected rays should actually take the same position as the extreme operative rays with the third position of the grating, it was necessary to determine the place in the radiometer-vane which with the third position is hit by the extreme ultra-red rays. As the intensity of the dark heat-rays diminishes as their distance from the red end of the spectrum decreases, and as the effectiveness of the rays falling upon the radiometer-vane increases with the distance from the torsion-axis of the vane, the torsion-apparatus was placed so that the extreme most sensitive edge of the vane was turned towards the red end of the investigated spectrum. If then the rays falling upon the vane called forth any motion at all, it was certain that effective rays fell also upon the extreme edge of the vane. On this account, with the second position of the grating the directly reflected image of the slit was adjusted so that the vertical thread of the cross coincided with the edge of the vane. The

irradiation of the part of the glass bulb of the torsion-apparatus nearer to the visible spectrum, which might possibly have produced a motion, was cut off by the sheet-metal case surrounding the apparatus.

To give the grating its third position, it was first placed so that the motion of the torsion-apparatus was very slight; then the finer adjustment was made by means of the micrometer-screw attached to the turning-arm of the stand. Very slight deflections of the radiometer-vane could be easily and rapidly multiplied by measured interruption and restoration of the irradiation. In the first position of the grating, in which no action was shown, it could with certainty be asserted that the extreme effective rays fell upon the boundary edge of the vane. In this position the reading *c* was made.

IV. *Results.*

Unfortunately, on account of unfavourable weather, but very few measurements could be effected. These gave the following results on starting from the red end of the first spectrum and advancing steadily to the place where the action of the apparatus ceased:—

	<i>a.</i>	<i>b.</i>	<i>c.</i>	λ .
July 25, with iodine solution.	139° 40' 0"	159° 0' 0"	130° 21' 55"	millim. 0·0013281
Aug. 7, " " "	167 30 0·0	183 57 47·0	154 57 19·4	0·0013658
" with ebonite	165 46 39·9	184 27 45·8	154 38 56·6	0·0013834

The situation of the extreme ultra-red rays fell here throughout in the red of the second spectrum, as one could easily be convinced by inserting a red glass plate, since this absorbed the blue end of the third spectrum, which coincided with the red end of the second.

When the grating was rotated still further, there came first a narrow strip of insensitvity; very soon, however, action of the dark rays upon the torsion-apparatus made its appearance. It commenced within the luminous spectrum of the second order, and extended to the extreme end of this spectrum, where the transition to the dark part of the spectrum of the second order rendered separation of the two spectra no longer possible.

It is therefore proved that rays occur in the sun's spectrum of which the wave-length is double that of the extreme visible

red rays; consequently its approximate value is

$$\lambda = 0.00152 \text{ millim.}$$

In order to determine the breadth of the ineffective strip, the wave-length of the first rays again effective was measured, with the following result:—

	<i>a.</i>	<i>b.</i>	<i>c.</i>	λ .
Aug. 7, with iodine solution.	165° 3' 32.0"	183° 31' 21.4"	153° 34' 21.5"	millim. 0.0013908
„ „ ebonite	166 25 14.3	182 41 9.3	153 13 51.5	0.0013864

The insensitive band extended therefore, on the employment of the iodine solution, from $\lambda = 0.0013658$ to $\lambda = 0.0013908$ millim., and, with the ebonite plate, from $\lambda = 0.0013834$ to $\lambda = 0.0013864$ millim. On the reason of this insensitiveness nothing exact can at present be stated. There are, however, three possibilities: either a group of Fraunhofer's lines exist at this place; or both the plate of ebonite and the iodine solution are adiathermanous for rays of this particular refrangibility; or the lampblack does not absorb them. The reason that the band is much broader when iodine solution is employed than with ebonite would, with all three explanations, be found in the circumstance that the iodine solution is, on the whole, less transparent at this part of the spectrum than the ebonite.

V. *Practical and Theoretical Limits.*

In our investigation we met with the practical difficulty that the dark rays of the spectrum of the first order coincided with those of the second. Although we have not at present the means of separating these two kinds of rays, it is very probable that, by careful investigation of the diathermancy of various materials, a substance will be discovered impervious to the first ultra-red rays and transmitting only the rays from a certain great wave-length onwards.

Now our arrangement above described is very convenient for the investigation of the diathermancy of any substance, since one has only to place the substance in question before the torsion-apparatus to ascertain with facility, on rotating the grating, all the parts of the spectrum at which the substance transmits the rays. Nay, even a quantitative determination of the diathermancy could be deduced, at least approximately, from the strength of the deflection produced.

If in this way we should find a substance possessing the desired property, and were to insert it in the path of the rays in our arrangement, it would separate for us the thermal spectra of the first and second orders just as the ebonite plate divided the luminous from the dark spectrum.

A second circumstance that might set a limit to the further investigation of the extreme ultra-red rays after the method we have chosen would be that of the lampblack on the radiometer-vane not possessing the requisite absorptive power for rays of so great a wave-length. If this should turn out to be the case, another substance will have to be selected, instead of lampblack, for the radiometric substance, possessing greater sensitiveness for the rays in question. Thus it is, perhaps, not impossible to advance beyond these practical limits of the investigation.

The case stands otherwise, however, with a limitation resulting from the theory, which follows directly from the formula (p. 240). Since, namely,

$$\lambda = d [\sin \delta + \sin (\delta + \delta_1)],$$

and $\sin \delta + \sin (\delta + \delta_1)$ can only become equal to 2 at the most (a limit, moreover, never to be reached in practice), λ can never become greater than $2d$. If still greater wave-lengths occurred in the sun's radiation, these rays would not appear in the interference spectrum, and to verify their existence by our method would be impossible. Now, as the greater d is made the smaller does the breadth of the spectrum become, this limitation of the method is a very serious one, because the accuracy of the determination essentially depends on the breadth of the spectrum. With the grating-mirror employed by us, this maximum value of λ would amount to

$$\lambda_m = 0.002970,$$

and therefore would be a quantity of absolutely the same order as the value of λ found from observation.

If, then, rays of so great a wave-length really do occur in the sun, this method, like every other that rests on the examination of a diffraction-spectrum, would not be capable of demonstrating them. Provisionally, however, it will be possible, by perfecting the means in the way above indicated, to demonstrate by our method the existence in the spectrum of the sun of rays of still greater wave-length than those found by us.

XXXV. On Effects of Retentiveness in the Magnetization of Iron and Steel.

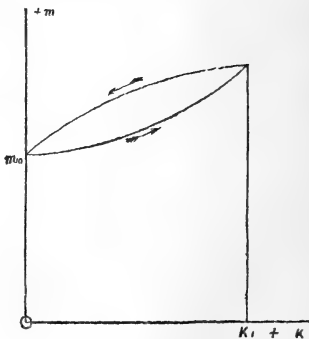
To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I BEG leave to forward to you for insertion in your valuable Journal the following note.

In the Proceedings of the Royal Society, vol. xxxiv. no. 220, p. 39, there may be found a preliminary notice by Mr. J. A. Ewing, communicated by Sir W. Thomson, received May 6, 1882 entitled, "On Effects of Retentiveness in the Magnetization of Iron and Steel," the first part of which is very closely related to a paper published by myself, Dec. 6, 1880, in the *Freiburger Berichte*, vol. viii., and reprinted in Wiedemann's *Annalen*, vol. xiii. pp. 141-164 (1881). In this paper the fundamental fact redescribed by Mr. Ewing was described by me a year before as follows:—

"A permanent moment m_0 may have been produced in an iron wire by the action of a longitudinal magnetizing force K_1 . Next let the wire be subjected to magnetizing forces which first increase continuously from 0 to K_1 and then decrease continuously from K_1 to 0; then, for the same magnetizing force K , the magnetic moment of the wire will be found greater when K is increasing than when it is decreasing. After some repetitions of the operation the wire will be found in a stationary state, in which for $K=0$ it always has the same moment m_0 and for $K=K_1$ the same moment $m_0 + m_1$. If, therefore, the wire being in this condition, its moment is represented graphically as a function of the magnetizing force K , a closed curve C will be got as represented in the figure, in which the ascending and the descending branch are marked by arrows; except the minimum (0) and maximum (K_1) value of K , two values of m correspond to every value of K "*.



* Observations included in the above statement are to be found in a paper by Fromme (*Wied. Ann.* iv. pp. 102-105, 1878); but the statement is not derived from the observations, and their bearing on other phenomena is not recognized.

In a footnote of my paper the following remark may be found:—"With this fact certain phenomena concerning the effect of torsion on the mag-

In § 1 of my paper it is shown that the integral $-\int mdK$ extended over the curve C represents in absolute measure the work done on the magnet during a cycle. In §§ 2, 6, 7 it is explained how the above phenomenon and this theorem are connected with the heat produced in a mass of iron by changes of magnetization, with the damping action exerted by a plate of iron on a magnet swinging over the plate*, and with the experiments of Christie† and Barlow‡. §§ 3 and 4 contain measurements concerning the described phenomenon for wires of different dimensions, and discussions of the results obtained. Lastly, in § 8 the analogy of the described phenomenon with the friction of solids is pointed out.

Poisson|| has given another theory of the damping action exerted by an iron plate on a magnet swinging over it. In Poisson's theory this action is made to depend upon a function of time; while in my theory the damping action depends on a merely statical phenomenon. In the experiments which F. Himstedt¶ has made to test my theory, the greatest part of the damping effect is shown to depend certainly on the action explained in my theory. Indeed, in these experiments, (1) the logarithmic decrement of the swinging magnet was found to be independent of the vibration-time, if this was altered by altering the moment of inertia of the swinging system; (2) about 0·84 of the observed damping action was calculated from merely static experiments; and reasons are given in F. Himstedt's paper that the remainder did not depend upon the Poisson effect.

In the physical laboratory of the Freiburg University an experimental inquiry has been executed by L. Hönig, concerning the connexion of the described phenomenon with the heat actually evolved in iron wires by changes of magnetization. The results of this investigation will soon be published in Wiedemann's *Annalen*.

I remain, Gentlemen,

Yours respectfully,

Freiburg i B., February 24, 1883.

E. WARBURG.

netic moment of an iron wire observed by Sir W. Thomson (Phil. Trans. clxx. pp. 68-72, 1879) and independently by myself are connected; probably also observations of E. Cohn (Wied. Ann. vi. pp. 388-392) on the thermoelectric properties of stretched iron wires."

Results similar to those obtained by me for iron wires have since been found (but not yet published) in cobalt and nickel by A. Ehret.

* Seebeck, Pogg. Ann. vii. p. 203, and xii. p. 352.

† Phil. Trans. 1825, part 1, pp. 347-417.

‡ Ibid. pp. 347-417. The last two points have not been considered by Mr. Ewing.

|| *Mémoires de l'Académie*, 1823, p. 467.

¶ Wied. Ann. vol. xiv. p. 483.

XXXVI. *On the Meaning of "Force."*
 By the Rev. MAXWELL H. CLOSE, M.A.

To the Editors of the Philosophical Magazine and Journal.

University Club, Dublin,
 March 9, 1883.

GENTLEMEN,

PROFESSOR Lamb's communication, on the Basis of Statics, in the current number of the Philosophical Magazine, is intended to promote the interests of the learner in dynamics. Will you permit me to point out, with the same object in view, that the word "Force" has not yet acquired a definite invariable meaning in the science of dynamics? Until lately the learner was very seriously incommoded by the fact that the name "Force" was applied, not only to force proper, or F , but also to energy, or $\int F ds$, these two being, as is universally recognized, totally disparate. This practice has been happily discontinued, or almost entirely so; but it is not, as far as I know, acknowledged, though it must have been perceived by some, that a similar one still very generally obtains. In accordance with Newton's rather free usage of "*vis*," the name "Force" is still applied not only to force proper, or F , but also to what we shall call on the present occasion Impulsion, or $\int F dt$, these two being as completely disparate as force proper and energy.

Clerk Maxwell designates $\int F dt$ by Impulse; which name, however, usually implies suddenness, intensity, and shortness of action, like that of impact. To avoid this association we shall now vary this name to Impulsion. It will conduce to brevity and convenience, and will answer the present purpose perfectly well, to suppose F constant and write Ft for $\int F dt$.

Of course in the first place "force" is applied to force proper. This is simple pressure or tension, which can be measured in pounds weight. It is represented by F in $Ft = mv$; kinetically, therefore, it is the time-rate of change of momentum (energetically it is F in $Fs = \frac{1}{2}mv^2$, and the space-rate of change of energy). But it will be seen from the following that "force" is often used for impulsion, or Ft , the correlate and numerical equivalent of momentum.

(a) Whoever translates Newton's Second Law of Motion into English, rendering "*vis motrix*" by "force," and adopting the statement as from Newton for himself, is using "force" for impulsion. From the exposition appended to Law II. it will be seen that "*vis motrix*" means, not F , but Ft ; for it is something which may be "*sive simul et semel, sive gradatim et successive impressa*." These words would be without meaning

if applied to force proper, but are quite appropriate to impulsion. And, agreeably to this, "*mutatio motûs*" means amount, not rate, of change of momentum; for in the exposition it appears as "*motus*," which may be added to (&c.) that which may have been originally possessed by the body. It is implied by some writers and expressly stated by others, who have not considered the exposition, that the change of momentum of Law II. is *rate* of change; they have been led to say this through the still existing ambiguity of "force." It is actually the case that, in consequence of this ambiguity, competent dynamicists unconsciously differ as to what is the subject matter of one of the fundamental Laws of Motion!

(b) When force is defined as that which produces momentum, or change of momentum, either "force" means impulsion, or else a very incongruous statement is made respecting force proper. Force proper, being the rate of change of momentum, cannot be the cause of momentum—just as the death-rate in some city for a certain week was not the cause of the aggregate of deaths in that week, and as velocity, the rate of displacement, is not that which produces the displacement, and as density, the volume-rate of mass, is not the cause of mass. Some other considerations show that force proper must not be called the cause of momentum: *e. g.* if we call it so we must, for a similar reason, also say that it is the cause of kinetic energy; so that the same cause may have two totally disparate effects, which is absurd. In accordance with Newton's language in the exposition of Law II., we may style impulsion the generator or cause of momentum; though it might be perhaps more correct to say that it is the power of producing momentum, just as energy is the power of performing work.

(c) When, as frequently occurs, force is said to be *expended*, either "force" means impulsion, or else a very incongruous statement is made respecting force proper. Under statical circumstances there is clearly no *expenditure* of force; and this is equally so in a kinetical operation. A force of one pound is applied to an immovable obstacle; an equal force is simultaneously applied to a free mass; at the end of some period of time both forces are all there, as at the beginning; there has been no expenditure of either in any ordinary non-transcendental sense of the word. That the point of application of the latter is not where it was at first is quite irrelevant to the present question. Force proper, or the rate of production of momentum, is not expended in producing the momentum, any more than the death-rate in some city is expended in producing the mortality therein. It is the impulsion, or Ft , which is expended *pari passu* with the produc-

tion of momentum, just as potential energy, or Fs , is expended in the performance of work.

(d) It is sometimes said that Newton has proved the composition of the effects of conjoint forces in Corollary 1 to the Laws of Motion. "Forces" here means impulsions; for it is only with these that that corollary deals. This appears not only *à priori*, because force proper does not produce motion, but also from the reference in that corollary to Law II., which must be read in the light of Newton's own exposition of it quoted above. If the times of the two impulsions be equal, then (but not otherwise) are the sides of the parallelogram proportional to the forces proper concerned; and the composition of these *follows from* Cor. 1. Though this additional step was so easy to Newton that he has unconsciously taken it for granted in Cor. 2, which, in the discussion, though not in the enunciation, deals with forces proper, yet it is an additional step, and should not be ignored; for it has, not unreasonably though perhaps unnecessarily, presented difficulty to distinguished dynamicists. The composition of forces proper does not follow immediately from that of velocities. Newton's argument, as clearly drawn out, consists of three stages, viz.:—1, the composition of velocities (kinematical); 2, the composition of impulsions (kinetical); 3, the composition of forces proper (statical). The second is involved in the first; but the third has to be deduced from the second.

(e) Sometimes F , as in $Ft = mv$, is spoken of as though it were impulsion of unit of time, and not merely numerically equivalent thereto and measurable thereby. Similarly v , or velocity, is sometimes spoken of as the space described in unit of time. In this and other cases there is no danger of any misapprehension on the part of the learner; but it is otherwise in the case of force. From frequent repetition the above form of speech has sometimes solidified, as it were, into a quasi-ontological principle, by which force and impulsion of unit of time are identified—these two being totally disparate, as much so as force proper and energy of unit of space. It is not easy to criticise satisfactorily the phrase "time-integral of force," which may have originated only as a brief and convenient, though acknowledgedly loose, expression. But, considering the insufficient recognition of the complete disparateness of force and impulsion, we may be allowed to suspect that in this expression it is not that dt is understood, but that "force" originally meant, and still means, the differential of impulse, or $F dt$; which would quite fall in with what we have just been complaining of. At any rate this expression tends to keep up the obscurity of distinction between force and impulsion or its element or its differential.

We may here observe (though this is a different point) that the expressions "time-integral of force," for impulsion, and "space-integral of force," for potential energy, have in some cases, from frequent use, reacted upon the users of them ; so that we find impulsion called the "total force in a finite time," and potential energy the "sum of the tensions," "force" and "tensions" being force and forces proper. The competent writers now alluded to have inadvertently allowed themselves to use expressions precisely analogous to the statement that the area of a plane curve is the sum of the ordinates. It can be only in some transcendental sense that a force or pressure, supposed constant for simplicity, which has existed statically for a given time, may be said to be the sum of the applications of force in each second of that time ; and this is equally true if the force have been existing kinetically.

What we may call the mathematical analogy between energy, or Fs , and impulsion, or Ft , is complete, notwithstanding the metaphysical considerations involved in the difference between their factors s and t . Each is of two dimensions ; while force, or F , is of one. F being measurable in pounds, Fs is measurable in foot-pounds, and Ft in second-pounds. Each is equally disparate from F ; though very dissimilarly so, since they are disparate from each other. As Fs is the power of performing work, so Ft is the power of producing momentum. Neither had a name until the present century. It was this that gave room to the remarkable controversy concerning the true measure of the "moving force" of a body in motion which went on for over forty years in the last century, "to the great scandal of science" as Montucla says. Each needs a distinctive appropriated name, as much as the other. One, viz. Ft , has not yet acquired a universally recognized proper name ; but the sooner it does so the better for the learner, and also for the science of dynamics, which then need not be guilty of the rather unscientific proceeding of occasionally giving more than one meaning to "Force."

Your obedient servant,

M. H. CLOSE.

XXXVII. *On the Number of Fractions contained in any "Farey Series" of which the Limiting Number is given.* By J. J. SYLVESTER*.

A *FAREY series* ("suite de Farey") is a system of all the unequal vulgar fractions arranged in order of magnitude, the numerator and denominator of which do not exceed a given number.

The first scientific notice of these series appeared in the

* Communicated by the Author.

Philosophical Magazine, vol. xlvii. (1816) pp. 385, 386. In 1879 Mr. Glaisher published in the Philosophical Magazine (pp. 321–336) a paper on the same subject containing a proof of their known properties, an important extension of the subject to series in which the numerators and denominators are subject to distinct limits, and a bibliography of Mr. Goodwyn's tables of such series. Finally, in 1881 Sir George Airy contributed a paper also to the Philosophical Magazine of that year, in which he refers to a table calculated by him "some years ago," and printed in the Selected Papers of the Transactions of the Institution of Civil Engineers, which is in fact a Farey table with the logarithms of the fractions appended to each of them. Previous tables had only given the decimal values of such fractions. The drift of this paper is to point out a caution which is necessary to observe in the use of such tables, and which limits their practical utility: this arises from the fact of the differences receiving a very large augmentation in the immediate neighbourhood of the fractions which are a small aliquot part of unity—a fact which may be inferred *à priori* from the well-known law discovered by Farey applicable to those differences, but to which the author of the paper makes no allusion.

In addition to the tables of Farey series by Goodwyn, Wucherer, an anonymous author mentioned in the Babbage Catalogue, and Gauss, referred to by Mr. Glaisher in his Report to the Bradford Meeting of the British Association (1873), may be mentioned one contained in Herzer's *Tabellen* (Basle, 1864) with the limit 57, and another in Hrabak's *Tabellen-Werk* (Leipsic, 1876), in which the limit is taken at 50.

The writers on the theory are:—Cauchy (as mentioned by Mr. Glaisher), who inserted a communication relating to it in the *Bulletin des Sciences par la Société Philomathique de Paris*, republished in his *Exercices de Mathématiques*; Mr. Glaisher himself (*loc. cit.*); M. Halphen, in a recent volume of the Proceedings of the Mathematical Society of France; and M. Lucas, in the next following volume of the same collection. I am indebted to my friend and associate Dr. Story for these later references.

For theoretical purposes it is desirable to count $\frac{1}{2}$ as one of the fractions in a Farey series. The number of such fractions for the limit j then becomes identical with the sum of the *totients* of all the natural numbers up to j inclusive—a totient to x (which I always denote by τx) meaning the number of numbers less than x and prime to it. Such sum, *i. e.*

$\sum_{x=0}^{\infty} \tau x$, I denote by Tj . My attention was called to the subject by this number Tj expressing the number of terms in a

function whose residue (in Cauchy's sense) is the generating function to any given simple denominator (see footnote to American Journal of Mathematics, vol. v. p. 123); and I became curious to know something about the value of Tj . I had no difficulty in finding a functional equation which serves to determine its limits (see Johns Hopkins University Circular, Jan. and Feb. 1883). The most simple form of that equation (omitted to be given in the Circular) is

$$Tj + T\frac{j}{2} + T\frac{j}{3} + T\frac{j}{4} + T\frac{j}{5} + \dots = \frac{j^2 + j}{2},$$

(where, when x is a fraction, Tx is to be understood to mean Tj , j being the integer next below x); and from this it is not difficult to deduce by strict demonstration that Tj/j^2 , when j increases indefinitely, approximates indefinitely near to $3/\pi^2$.

I have subsequently found that if ux be used to denote the sum of all the numbers inferior and prime to x , and $Uj = \sum_{x=0}^{x=j} ux$,

$$Uj + 2U\frac{j}{2} + 3U\frac{j}{3} + 4U\frac{j}{4} + \dots = \frac{j(j+1)(j+2)}{3}$$

(where Ux , when x is a fraction, means the U of the integer next inferior to x). From this equation it is also possible to prove that Uj/j^3 , when j becomes indefinitely great, approximates to $1/\pi^2$. Uj , it may be well to notice, is the sum of all the numerators of the fractions in a Farey series whose limit is j , just as Tj is the number of these fractions.

In the annexed Table the value of τx (the totient), of Tx (the sum-totient), and of $3/\pi^2 \cdot x^2$ is calculated for all the values of x from 1 to 500; and the remarkable fact is brought to light that Tx is always greater than $3/\pi^2 \cdot x^2$ (the number opposite to it), and less than $3/\pi^2 \cdot (x+1)^2$, the number which comes after the former one in the same table.

I have calculated in my head the first few values of Ux , and find (if I have made no mistake) that it obeys an analogous law, viz. is always intermediate between $1/\pi^2 \cdot x^3$ and $1/\pi^2 \cdot (x+1)^3$.

It may also be noticed that when n is a prime number, Tn is always nearer, and usually very much nearer, to the superior than to the inferior limit—as might have been anticipated from the circumstance that, when this is the case, in passing from $n-1$ to n the T receives an augmentation of $n-1$, whereas its average augmentation is only $\frac{3}{\pi^2}(2n-1)$.

In like manner and for a similar reason, when n contains several small factors Tn is nearer to the inferior than to the superior limit. For instance, when $n=210$, $Tn=13414$ and $3/\pi^2 \cdot n^2=1340479$.

TABLE of Totients, of Sum-totients, and of $3/\pi^2$ into the Squares of all the Numbers from 1 to 500 inclusive.

$$\left[\frac{3}{\pi^2} = \cdot 30396355 \right].$$

n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2} n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2} n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2} n^2$
1	1	1	·30	49	42	754	729·82	97	96	2902	2860·00
2	1	2	1·22	50	20	774	759·91	98	42	2944	2919·27
3	2	4	2·74	51	32	806	790·61	99	60	3004	2979·15
4	2	6	4·86	52	24	830	821·92	100	40	3044	3039·64
5	4	10	7·60	53	52	882	853·83	101	100	3144	3100·73
6	2	12	10·94	54	18	900	886·36	102	32	3176	3162·44
7	6	18	14·90	55	40	940	919·49	103	102	3278	3224·75
8	4	22	19·46	56	24	964	953·23	104	48	3326	3287·67
9	6	28	24·62	57	36	1000	987·58	105	48	3374	3351·20
10	4	32	30·40	58	28	1028	1022·54	106	52	3426	3415·34
11	10	42	36·78	59	58	1086	1058·10	107	106	3532	3480·08
12	4	46	43·77	60	16	1102	1094·27	108	36	3568	3545·44
13	12	58	51·37	61	60	1162	1131·05	109	108	3676	3611·40
14	6	64	59·58	62	30	1192	1168·44	110	40	3716	3677·96
15	8	72	68·39	63	36	1228	1206·43	111	72	3788	3745·14
16	8	80	77·81	64	32	1260	1245·03	112	48	3836	3812·92
17	16	96	87·84	65	48	1308	1284·25	113	112	3948	3881·31
18	6	102	98·48	66	20	1328	1324·07	114	36	3984	3950·31
19	18	120	109·73	67	66	1394	1364·49	115	88	4072	4019·92
20	8	128	121·58	68	32	1426	1405·53	116	56	4128	4090·14
21	12	140	134·05	69	44	1470	1447·17	117	72	4200	4160·96
22	10	150	147·12	70	24	1494	1489·42	118	58	4258	4232·39
23	22	172	160·79	71	70	1564	1532·28	119	96	4354	4304·43
24	8	180	175·08	72	24	1588	1575·75	120	32	4386	4377·08
25	20	200	189·98	73	72	1660	1619·82	121	110	4496	4450·33
26	12	212	205·48	74	36	1696	1664·51	122	60	4556	4524·19
27	18	230	221·59	75	40	1736	1709·80	123	80	4636	4598·66
28	12	242	238·31	76	36	1772	1755·69	124	60	4696	4673·74
29	28	270	255·63	77	60	1832	1802·20	125	100	4796	4749·43
30	8	278	273·56	78	24	1856	1849·31	126	36	4832	4825·72
31	30	308	292·11	79	78	1934	1897·04	127	126	4958	4902·63
32	16	324	311·26	80	32	1966	1945·37	128	64	5022	4980·14
33	20	344	333·01	81	54	2020	1994·31	129	84	5106	5058·26
34	16	360	351·38	82	40	2060	2043·85	130	48	5154	5136·98
35	24	384	372·35	83	82	2142	2094·01	131	130	5284	5216·32
36	12	396	393·93	84	24	2166	2144·77	132	40	5324	5296·26
37	36	432	416·12	85	64	2230	2196·14	133	108	5432	5376·81
38	18	450	438·92	86	42	2272	2248·12	134	66	5498	5457·97
39	24	474	462·32	87	56	2328	2300·70	135	72	5570	5539·74
40	16	490	486·34	88	40	2368	2353·90	136	64	5634	5622·11
41	40	530	510·96	89	88	2456	2407·70	137	136	5770	5705·09
42	12	542	536·19	90	24	2480	2462·10	138	44	5814	5788·68
43	42	584	562·02	91	72	2552	2517·12	139	138	5952	5872·88
44	20	604	588·47	92	44	2596	2572·75	140	48	6000	5957·69
45	24	628	615·52	93	60	2656	2628·98	141	92	6092	6043·10
46	22	650	643·19	94	46	2702	2685·82	142	70	6162	6129·12
47	46	696	671·45	95	72	2774	2743·27	143	120	6282	6215·75
48	16	712	700·33	96	32	2806	2801·33	144	48	6330	6302·99

Table (continued).

n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2} n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2} n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2} n^2$
145	112	6442	6390.83	198	60	11954	11916.59	251	250	19274	19150.01
146	72	6514	6479.29	199	198	12152	12037.26	252	72	19346	19302.90
147	84	6598	6568.35	200	80	12232	12158.54	253	220	19566	19456.40
148	72	6670	6658.02	201	132	12364	12280.43	254	126	19692	19610.51
149	148	6818	6748.29	202	100	12464	12402.93	255	128	19820	19765.23
150	40	6858	6839.18	203	168	12632	12526.03	256	128	19948	19920.56
151	150	7008	6930.67	204	64	12696	12649.75	257	256	20204	20076.49
152	72	7080	7022.77	205	160	12856	12774.07	258	84	20288	20233.03
153	96	7176	7115.48	206	102	12958	12899.00	259	216	20504	20390.18
154	60	7236	7208.80	207	132	13090	13024.54	260	96	20600	20547.94
155	120	7356	7302.72	208	96	13186	13150.68	261	168	20768	20706.30
156	48	7404	7397.26	209	180	13366	13277.43	262	130	20898	20865.28
157	156	7560	7492.40	210	48	13414	13404.79	263	262	21160	21024.86
158	78	7638	7588.15	211	210	13624	13532.76	264	80	21240	21185.05
159	104	7742	7684.51	212	104	13728	13661.34	265	208	21448	21345.84
160	64	7806	7781.47	213	140	13868	13790.52	266	108	21556	21507.25
161	132	7938	7879.04	214	106	13974	13920.32	267	176	21732	21669.26
162	54	7992	7977.22	215	168	14142	14050.72	268	132	21864	21831.88
163	162	8154	8076.01	216	72	14214	14181.73	269	268	22132	21995.11
164	80	8234	8175.41	217	180	14394	14313.34	270	72	22204	22158.95
165	80	8314	8275.41	218	108	14502	14445.57	271	270	22474	22323.39
166	82	8396	8376.02	219	144	14646	14578.40	272	128	22602	22488.44
167	166	8562	8477.24	220	80	14726	14711.84	273	144	22746	22654.10
168	48	8610	8579.07	221	192	14918	14845.89	274	136	22882	22820.37
169	156	8766	8681.50	222	72	14990	14980.54	275	200	23082	22987.25
170	64	8830	8784.55	223	222	15212	15115.81	276	88	23170	23154.73
171	108	8938	8888.20	224	96	15308	15251.68	277	276	23446	23322.82
172	84	9022	8992.46	225	120	15428	15388.16	278	138	23584	23491.52
173	172	9194	9097.33	226	112	15540	15525.25	279	180	23764	23660.83
174	56	9250	9202.80	227	226	15766	15662.94	280	96	23860	23830.75
175	120	9370	9308.88	228	72	15838	15801.24	281	280	24140	24001.27
176	80	9450	9415.57	229	228	16066	15940.15	282	92	24232	24172.40
177	116	9566	9522.87	230	88	16154	16079.67	283	282	24514	24344.14
178	88	9654	9630.78	231	120	16274	16219.80	284	140	24654	24516.49
179	178	9832	9739.29	232	112	16386	16360.53	285	144	24798	24689.44
180	48	9880	9848.42	233	232	16618	16501.87	286	120	24918	24863.00
181	180	10060	9958.15	234	72	16690	16643.82	287	240	25158	25037.18
182	72	10132	10068.49	235	184	16874	16786.38	288	96	25254	25211.96
183	120	10252	10179.44	236	116	16990	16929.55	289	272	25526	25387.34
184	88	10340	10290.99	237	156	17146	17073.32	290	112	25638	25563.34
185	144	10484	10403.15	238	96	17242	17217.70	291	192	25830	25739.94
186	60	10544	10515.92	239	238	17480	17362.70	292	144	25974	25917.15
187	160	10704	10629.30	240	64	17544	17508.30	293	292	26266	26094.97
188	92	10796	10743.29	241	240	17784	17654.51	294	84	26350	26273.40
189	108	10904	10857.88	242	110	17894	17801.32	295	232	26582	26452.43
190	72	10976	10973.09	243	162	18056	17948.74	296	144	26726	26632.07
191	190	11166	11088.90	244	120	18176	18096.77	297	180	26906	26812.32
192	64	11230	11205.31	245	168	18344	18245.41	298	148	27054	26993.18
193	192	11422	11322.34	246	80	18424	18394.66	299	264	27318	27174.65
194	96	11518	11439.97	247	216	18640	18544.51	300	80	27398	27356.72
195	96	11614	11558.21	248	120	18760	18694.97	301	252	27650	27539.40
196	84	11698	11677.06	249	164	18924	18846.04	302	150	27800	27722.69
197	196	11894	11796.52	250	100	19024	18997.72	303	200	28000	28906.59

Table (continued).

n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$
304	144	28144	28091.10	357	192	38822	38739.85	410	160	51210	51096.27
305	240	28384	28276.21	358	178	39000	38957.18	411	272	51482	51345.83
306	96	28480	28461.93	359	358	39358	39175.13	412	204	51686	51595.99
307	306	28786	28648.26	360	96	39454	39393.68	413	348	52034	51846.76
308	120	28906	28835.20	361	342	39796	39612.83	414	132	52166	52098.14
309	204	29110	29022.75	362	180	39976	39832.60	415	328	52494	52350.12
310	120	29230	29210.90	363	220	40196	40052.97	416	192	52686	52602.72
311	310	29540	29389.66	364	144	40340	40273.95	417	276	52962	52855.92
312	96	29636	29589.03	365	288	40628	40495.54	418	180	53142	53109.73
313	312	29948	29779.01	366	120	40748	40717.74	419	418	53560	53364.15
314	156	30104	29969.59	367	366	41114	40940.55	420	96	53656	53619.17
315	144	30248	30160.79	368	176	41290	41163.96	421	420	54076	53874.80
316	156	30404	30352.59	369	240	41530	41387.98	422	210	54286	54131.04
317	316	30720	30545.00	370	144	41674	41612.61	423	276	54562	54387.89
318	104	30824	30738.01	371	312	41986	41837.85	424	208	54770	54645.35
319	280	31104	30931.64	372	120	42106	42063.69	425	320	55090	54903.42
320	128	31232	31125.87	373	372	42478	42290.15	426	140	55230	55162.09
321	212	31444	31320.71	374	160	42638	42517.21	427	360	55590	55421.39
322	132	31576	31516.16	375	200	42838	42744.87	428	212	55802	55681.26
323	288	31864	31712.22	376	184	43022	42973.15	429	240	56042	55941.76
324	108	31972	31908.88	377	336	43358	43202.04	430	168	56210	56202.86
325	240	32212	32106.15	378	108	43466	43431.53	431	430	56640	56464.57
326	162	32374	32304.03	379	378	43844	43661.63	432	144	56784	56726.89
327	216	32590	32502.52	380	144	43988	43892.34	433	432	57216	56989.82
328	160	32750	32701.62	381	252	44240	44123.65	434	180	57396	57253.36
329	276	33026	32901.32	382	190	44430	44355.58	435	224	57620	57517.50
330	80	33106	33101.63	383	382	44812	44588.11	436	216	57836	57782.26
331	330	33436	33302.55	384	128	44940	44821.25	437	396	58232	58047.62
332	164	33600	33504.08	385	240	45180	45055.00	438	144	58376	58313.58
333	216	33816	33706.22	386	192	45372	45289.35	439	438	58814	58580.16
334	166	33982	33908.96	387	252	45624	45524.32	440	160	58974	58847.34
335	264	34246	34112.31	388	192	45816	45759.89	441	252	59226	59115.14
336	96	34342	34316.27	389	388	46204	45996.07	442	192	59418	59383.54
337	336	34678	34520.84	390	96	46300	46232.86	443	442	59860	59652.54
338	156	34834	34726.01	391	352	46652	46470.25	444	144	60004	59922.16
339	224	35058	34931.80	392	168	46820	46708.25	445	352	60356	60192.38
340	128	35186	35138.19	393	260	47080	46946.87	446	222	60578	60463.22
341	300	35486	35345.19	394	196	47276	47186.09	447	296	60874	60734.66
342	108	35594	35552.80	395	312	47588	47425.91	448	192	61066	61006.70
343	294	35888	35761.01	396	120	47708	47666.35	449	448	61514	61279.36
344	168	36056	35969.83	397	396	48104	47907.39	450	120	61634	61552.62
345	176	36232	36179.26	398	198	48302	48149.04	451	400	62034	61826.49
346	172	36404	36389.30	399	216	48518	48391.30	452	224	62258	62100.97
347	346	36750	36599.95	400	160	48678	48634.17	453	300	62558	62376.06
348	112	36862	36811.21	401	400	49078	48879.64	454	226	62784	62651.75
349	348	37210	37023.07	402	132	49210	49121.73	455	288	63072	62928.05
350	120	37330	37235.54	403	360	49570	49366.42	456	144	63216	63204.97
351	216	37546	37448.61	404	200	49770	49611.72	457	456	63672	63482.48
352	160	37706	37662.30	405	216	49986	49857.62	458	228	63900	63760.61
353	352	38058	37876.59	406	168	50154	50104.14	459	288	64188	64039.35
354	116	38174	38091.50	407	360	50514	50351.26	460	176	64364	64318.69
355	280	38454	38307.01	408	128	50642	50598.99	461	460	64824	64598.64
356	176	38630	38523.12	409	408	51050	50847.33	462	120	64944	64879.20

Table (*continued*).

n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$	n	$\tau(n)$	$T(n)$	$\frac{3}{\pi^2}n^2$
463	462	65406	65160·36	476	192	68964	68870·85	489	324	72872	72684·07
464	224	65630	65442·14	477	312	69276	69160·52	490	168	73040	72981·65
465	240	65870	65724·52	478	238	69514	69450·81	491	490	73530	73279·84
466	232	66102	66007·51	479	478	69992	69741·70	492	160	73690	73578·63
467	466	66568	66291·11	480	128	70120	70033·20	493	448	74138	73878·04
468	144	66712	66575·31	481	432	70552	70325·31	494	216	74354	74178·05
469	396	67108	66860·13	482	240	70792	70618·03	495	240	74594	74478·67
470	184	67292	67145·55	483	264	71056	70911·35	496	240	74834	74779·90
471	312	67604	67431·58	484	220	71276	71205·29	497	420	75254	75081·73
472	232	67836	67718·22	485	384	71660	71499·83	498	164	75418	75384·18
473	420	68256	68005·46	486	162	71822	71794·98	499	498	75916	75687·23
474	156	68412	68293·32	487	486	72308	72090·73	500	200	76116	75990·89
475	360	68772	68581·78	488	240	72548	72387·10				

XXXVIII. *On Permanent Magnetism.*By R. H. M. BOSANQUET, *St. John's College, Oxford.**To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

I HAVE succeeded in completely determining, to a moderate approximation, all the quantities involved in the experiment mentioned in my paper in the March number of the *Philosophical Magazine*. I propose to give a short summary of the results, reserving a more complete account for a future occasion.

The external or air resistances were determined by magnetizing, in the bifilar, arrangements of soft iron similar to the steel. The resistance of the soft iron is small, and is allowed for by Rowland's table. The total resistance is the quotient of the magnetomotive force, ($4\pi Cn$), by the magnetic induction \mathfrak{B} .

A set of 18 pieces of soft iron similar to the pieces of the compound magnet gave for the total resistance of each piece ·454 centim.

$\mathfrak{B}=116$ gives from Rowland's table μ =about 400, whence $\frac{1\cdot58}{400}=.004$ is to be subtracted, and we have for the air resistance ·450 centimetre, or ·47 R. (R is not quite the same in the iron and steel.)

A long piece of soft iron, similar to the whole magnet joined up, gave

Total resistance = ·339 centim.

$\mathfrak{B}=553$ gives from Rowland's table $\mu=800$ nearly, whence $\frac{28.5}{800}$ is to be subtracted, $=.036$;

$$\begin{aligned}\text{Air resistance} &= .303 \text{ centim.} \\ &= .32 R \text{ say.}\end{aligned}$$

In the steel R is very nearly 1 centim.

With the above values for the air resistances we can determine the resistance of the steel from the ratio of deflections when joined and separate. Let r be the resistance of each piece. Then

$$\frac{18r + .32}{18(r + .47)} = \frac{\tan 13^\circ}{\tan 1^\circ.8} = .136,$$

whence

$$r = .0555;$$

and resistance of unit length

$$= \frac{r}{1.58} = \frac{1}{28.5},$$

whence

$$\mu = 28.5.$$

Direct Determination of Permeability (μ) of Steel Compound Magnet.

The magnet was enclosed in a paper case, joined up, and wound with 180 coils. The currents employed did not disturb the permanent magnetism.

Current.	Mean displacement.
.48 ampère.	$2^\circ.0$, from 12° to 14° .

(The deflection was really observed from zero 13° in opposite directions; and it amounted to $4^\circ.0$.)

$$\text{Change of moment} = 497.$$

(The moment of the bar joined up is 3120.)

$$\text{Total resistance} = 1.4 \text{ centim.}$$

$$\text{Subtracting air resistance} = .3 \text{ centim.}$$

$$\text{Resistance of steel} = 1.1 \text{ centim.}$$

$$\text{Resistance of unit length} = \frac{1.1}{28.5} = \frac{1}{26}.$$

$$\mu = 26^*.$$

* Since the above was written I have subjected the numbers to a careful scrutiny, and made another determination of μ directly, having discovered a possible source of error in the above. The resulting numbers all lie between 30 and 32.

The discrepancy between 26 and 28 is not greater than may be expected from the smallness of some of the observed quantities. These numbers agree very well with those given by Meyer for his first two sorts of hard steel, which, I gather, were in better condition than the others (Wiedemann's *Annalen*, xviii. p. 245). The values that agree best with each other are about

$$\begin{array}{l} k=2.1, \quad \mu=4\pi k=26.4 \\ \text{to} \\ k=2.5, \quad \mu= \quad 31.4. \end{array}$$

I propose to give some details in a future paper.

The result is, that if the magnetic induction in a permanent magnet be supposed to arise from a permanent magnetomotive force acting on a magnetic resistance, the resistance can be determined by the behaviour of the magnet on separation; and this resistance is numerically identical with that offered to external magnetomotive force.

XXXIX. *On the Errors of our Sensations: a Contribution to the Study of Illusion and Hallucination**. By EMILE YUNG, D.Sc.†

WHILE investigating, during the last few years, the question of animal magnetism, I have been led to pay some attention to the phenomena of illusion and hallucination. We know in a general way to how many errors our sense-organs are exposed; it is useful to ascertain these in one's self, to produce them in other people, and to try to correct them in every body; and it is necessary to determine them for each sense and for each individual. The same phenomenon is often appreciated in very different manners by different observers, or by the same observer at different periods. We should always adopt it as an absolute rule to observe the same fact several times under different subjective conditions before asserting its objective existence. And if it is desirable that this rule should be applied by well qualified observers, it ought,

* The present memoir gives only the commencement of a series of experiments undertaken upon the normal human subject with the purpose of checking and *measuring* the value of the testimony of our organs of sense. Dr. Yung begs that physicists and naturalists will have the goodness to communicate to him any isolated but positive observations that they may possess upon this subject.

† Translated from the *Bibliothèque Universelle, Archives des Sciences Physiques et Naturelles*, sér. 3, tome ix. p. 156.

à fortiori, always to be so by persons who have not learned to see and feel—that is to say, who do not know how to check by one another the data furnished by their sense-organs.

In fact we cannot scrutinize scientifically the mysteries of the external world except by the intermediation of the organs of the senses, and we only know of them what the latter convey to us. It is consequently of the first importance to us that these organs should not deceive us. Nevertheless two kinds of error are frequent, namely illusion and hallucination.

To make clear what is to follow, we may briefly recapitulate in what that complex phenomenon that we call a sensation actually consists. In every sensation we may distinguish three phases:—

1. The *impression*, which consists in the direct or indirect contact of the object with the *receptive apparatus*, or the peripheric extremity of our sensitive nerves. In order to fix our ideas let us assume (and this is in accordance with the recent data of physiology) that from the contact in question there results a movement, a shock, a vibration, which modifies the receptive apparatus.

2. The *transmission* of the impression (that is to say, of this movement, or vibration) through special organic conductors, the nervous tubes.

3. *Perception*, which is produced for the observer by a movement of reaction effected in the cellular nervous mass. Perception is *conscious* or *unconscious*, according as the movement has been transmitted to the cerebral cells, or only to those of the spinal marrow.

The nerves of special sense are unfitted to convey other impressions than those for which they have been specialized. Any mechanical, physical, or chemical disturbance of the optic nerve, for example, will show itself not by a perception of pain, but by a luminous perception. Under similar circumstances the auditory nerve will transmit a sonorous sensation &c. A special nerve may therefore transmit a sensation independently of the agent which normally excites it. In the most complete darkness we may experience luminous sensations; for this, as every one knows, it is sufficient to compress rather strongly the globe of the eye, and thus to agitate the retina.

We do not know the localities in the brain in which the movements communicated by luminous or sonorous vibrations become transformed into conscious sensations of light or sound; but it is probable that all the cells of the brain are not indifferently fitted for this purpose. It is very probable that there is in this case a division of physiological labour, in

accordance with which certain cell-groups are specially set apart for luminous transformations, other groups for sonorous transformations, &c.

If we assume for a moment that some disturbance or other (for example, a more considerable afflux of blood into the corresponding region of the brain) excites one of these cell-groups, there will result from it a conscious sensation, or the revival of an anterior sensation. And it is thus, under certain circumstances, by defect of judgment, or by defect of control, that we may be led to interpret as real luminous or sonorous sensations internal agitations, independent of any light or sound, of the optic or auditory perceptive centres.

In an analogous manner, in certain conditions of alteration of the nervous conductors, real objective impressions may be modified during their transmission to the brain. Hence come false perceptions of images, or of sounds, arising from a pathological condition of the optic or auditory nerves.

To sum up:—A sensation excited by an external object may be altered during its transmission to the brain; and a sensation not corresponding to any external object may originate in consequence of an internal disturbance of the sensitive nerves or of the perceptive centres themselves. In the former case the result has received the name of *illusion*; in the second we have to do with *hallucination*. Hallucination, says Prof. Ball, is a perception without an object.

The individual who thinks he sees a brigand armed to the teeth when he has before him only the trunk of a tree, or who sees a body in motion when the body which meets his eyes is really in repose, is under the influence of an illusion. Those who see phantoms pass in the dark, who hear celestial voices in the most absolute silence, or who believe that they touch angels or demons when no object strikes their tactile extremities, are under hallucination.

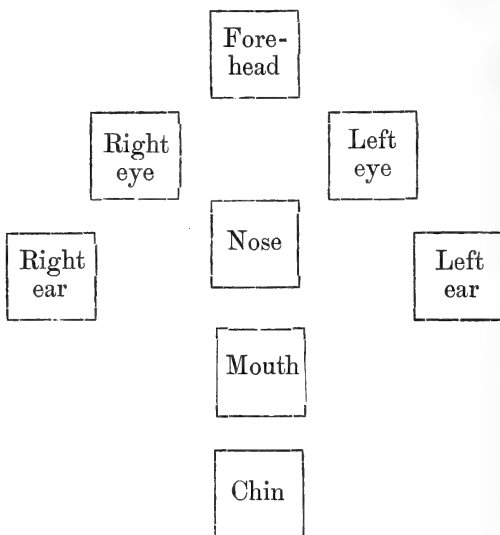
These, of course, are extreme examples; but within more restricted limits we are all exposed to similar errors which become the source of false interpretations in the observation of natural phenomena. We all know trustworthy persons who assert that they have experienced some sensation or other, upon which they furnish all the details one can ask of them, and who in reality have been the subjects of a hallucination. Facts of this kind are exceedingly frequent among the "good subjects" of the mesmerists—that is to say, among persons suffering from a hysterical, hypnotic, somnambolic, or other neurosis, and who, by the mere fact of their peculiar nervous condition, are much more exposed to the production of illusions or hallucinations.

I discard here the cases of neurosis, and shall only speak of the hallucinations which one may succeed in exciting in every individual. I must hope to be pardoned for the somewhat charlatanic proceedings adopted in the following experiments, in consideration of the interest of the results obtained.

FIRST SERIES.

Experiments made with the so-called Magnetized Card.*

Arrangement.—I take any eight cards from a pack and arrange them on a table in a certain figure to which I attribute very great importance. This figure, as shown in the following diagram, corresponds to the human face (1 card for the forehead, 2 for the eyes, 1 for the nose, 2 for the ears, 1 for the mouth, and 1 for the chin). Then, after having affected scepticism upon the question of the existence of “a



magnetic fluid,” I admit that the experiment which I am about to show to the persons present seems to me to prove that there are certain phenomena difficult to explain without a “fluid.” I then touch all the cards in order to impregnate them with “my fluid,” and declare myself ready to try the

* This card trick is very old and very well known. I do not know, however, that it has ever been made use of for methodical experiments. The trick itself was shown to me by my learned friend Dr. Léon Frédéricq, Professor of Physiology in the University of Liège. He witnessed some of the results described further on.

experiment with the first person who may desire to do so. It is indeed very rare, as will be seen immediately, for the experiment not to succeed, and this equally with incredulous persons and with those who believe in the fluid and in the fantastic tales of some mesmerists. I then pretend to place myself in magnetic relation with some person in the company, I apply my hand to his forehead, press his hand strongly, or perform some other absurdity of the same kind, and then request him to touch one of the cards of the figure without my knowledge. In order still more to convince the party that I do not see, I quit the room while he "magnetizes" the card.

At my return, an accomplice (for one is necessary) immediately indicates the card touched, without attracting anybody's attention, by scratching the corresponding part (nose, chin, eye, &c.) of his own face. Being thus instructed I perform an innocent comedy, which consists in pretending that I am going to recognize the card touched by means of a sensation (attraction, draught of air, quivering, odour, &c.) due no doubt to the fluid deposited by the person. In fact I make believe to experience this sensation from the card which has been indicated to me by my accomplice, as I have described. As I am never mistaken, every one is much astonished at the success obtained. How is it possible that the fact of the fingers having been placed for a moment upon this card has sufficed to communicate to it such a property?

Up to this point the experiment has nothing very remarkable about it. But if I beg the astonished person, who is still perhaps rather incredulous, to leave the room in his turn, and himself to seek for the card that I will magnetize in his absence, on his return he imitates the examination that he saw me make, he watches for the sensation that I have announced to him, and always, or nearly always, picks out a card. He thus becomes the subject of a true hallucination, seeing that, during his absence, I do not touch any card, and yet he supposes that he has undergone the shock, the vibration, the uncomfortableness, &c. which I have suggested to him.

If all the people present agree to testify that the card at which he fancies he has experienced a sensation is really that which I am supposed to have magnetized, he becomes confirmed in his notion, and I can make him repeat the same experiment several times over *with a constantly increasing success.*

Results.—To this experiment as just described I have subjected more than 100 people of different sexes and ages, most of them in good health, well educated, and attached to sci-

tific work, consequently not much inclined to excessive credulity. Of this number only 11 have refused to indicate a card, saying that, notwithstanding all the attention they could give to it, they *felt* absolutely nothing at any of the cards of the figure, neither the sensation indicated nor any other sensation. These 11 persons were men. All the rest experienced a sensation. They may be divided into three groups.

First Group.—32 persons forewarned that the card supposed to be magnetized would be recognized by the fact that, in passing the hand over it, it would produce a *shock* of variable intensity (sensible sometimes only in the tips of the fingers, sometimes in the whole hand, the arm, &c.), actually felt the shock. To the question, “Are you quite certain that you have not been the subject of a hallucination?” they answered that they were quite sure that they had felt something. Usually, however, the sensation is not very intense the first time; but if, as already described, the person is requested to repeat the experiment when a first trial has convinced him of the possibility of recognizing the magnetized card, it is invariably the case that on the second occasion the shock appears to him to be stronger, and its intensity only increases at each repetition of the experiment.

I have succeeded in exciting the belief in a shock generalized in the whole body in a young woman of twenty. I had myself, in performing the experiment, indicated the card that she had touched in my absence, by pretending to receive a general shock in the arms and legs and simulating the gestures which one makes in such a case. When her turn came she started in passing her hand over a certain card which I had not touched any more than the others, and asserted that she had received the shock announced not only in the limbs, but also in the chest. When I told her that the shock could never affect the chest, she replied “that she was quite sure she was not deceiving herself, for that her chest still pained her a little.”

Nine other persons in this group, two of them men, declared that besides the shock they had experienced “a slight oppression,” or various sensations, such, for example, as a certain difficulty in moving the fingers. One of them (a lady) whom I had induced to make the experiment at an evening party, told me the next day that, after my departure, she had tried in vain to play the piano, “because her right hand,” the one that had felt the imaginary shock, “was as if paralyzed, and was not even yet completely unstiffened.” It is to be remarked that these nine persons, although they had never been magnetized, had all been present at representations given by mesmerizers.

Except in the extreme cases just noticed, the shock was experienced only in the fingers or the wrist.

Some men of science who witnessed these experiments thought that there was no reason to regard the sensations experienced as true hallucinations, but only as due to complaisant simulation. In connexion with this I may remark that I mention here only the results obtained in the case of persons upon whose good faith I have the best reason to depend. My notes relate to a much larger number of subjects; but I eliminate all those upon whom I have only made a single experiment and those whom I do not know sufficiently well. Moreover the results which will be mentioned under the fourth group dispose of the hypothesis of simulation by their spontaneity.

Second Group.—I have succeeded in exciting hallucinations of sight by announcing that the magnetized card was revealed to me by a slight vacillation, or displacement, and inducing the person upon whom I was operating to look attentively at each of the cards until he saw one of them move. 12 persons out of 27 indicated a card from having seen it move. It is certain, as all the other witnesses of the experiment will testify, that none of the cards had changed its place. One person declared that “the card had raised itself;” another said that “the card had inclined itself with a wavering motion;” while a third, who had frequented the mesmerists and been put to sleep by them, asserted that at the moment when she looked at the card “she had felt that her eyes were invincibly attracted to it.” This phenomenon of the attraction of the looks inducing the movement of the whole body of the individual is very well known to mesmerists.

I would compare with these cases the numerous data that we possess with regard to observers who see certain movements of the stars, or other celestial phenomena, which have no real existence. Last year, when the famous comet, which every one remembers, was no longer visible, I happened to assert before six people that I still saw it, and at the same time I indicated to them the point in the heavens where I pretended to see it. Three of these persons saw it as I described it to them, but *indistinctly*; one of them even drew it, not such as he had actually seen it a few weeks before, but as his imagination, excited and guided by my assertions, showed it to him.

Here is another observation, which I made a few months ago. Near Roscoff, in Finistère, there is a large granite rock, firmly planted upon a small grass-plot on the shore, and known by the Celtic name of *Roch-roum*. Finding

myself at nightfall in the company of three people, and the conversation having turned upon the rocking-stones, some examples of which are known really to occur upon the coasts of Brittany, I asserted that Roch-roum itself rocked when it was gently pushed with the finger. Two of these persons immediately made the experiment and admitted with astonishment that they saw and felt it rock; and it gave the third some trouble to persuade them to the contrary. I have since several times repeated the experiment with rocks just as immovable as Roch-roum, and succeeded so well with certain people that they, repeating the experiment from time to time, have given the reputation of rocking-stones to blocks of stone which are in reality absolutely fixed. But we must return to our cards.

Third Group.—I repeated the experiment with the cards upon 20 persons, pretending that the touched card diffused a certain odour, which I sometimes specified exactly and sometimes indicated only in a vague manner. Thirteen persons (8 women and 5 men) responded by indicating a card, declaring in the first case that they had perceived the specified odour, and in the second case giving very various qualifications to the odour which they believed they had experienced—a sharp odour, a penetrating odour, a sweet smell like a perfume, &c. One person asserted that “the odour had stuck to her for some hours.” (See further on, the second series of experiments.)

Fourth Group.—Lastly, 18 persons out of 21 to whom I had mentioned a sensation of some kind without specially defining it indicated a card, and proved very fertile in different appreciations of the sensation which they believed they had experienced. It was indeed in this group that I obtained the most singular results.

A young girl (Louisa C., 19 years of age) indicated a card, and said that in passing her hand over it she had experienced “a great cold creeping in the back.” Another young girl (Ellen W., 18 years old) suddenly fell down backwards, as if yielding to a violent repulsion, on passing her hand over a card, also believing that this card had been magnetized.

Here each case is interesting; but it would take too long to report them in detail. I will only say that the imagination, while showing in some subjects an unheard of fecundity, often gives place to imitation; and although warned that they might expect all sorts of sensations, the subjects sometimes notify the sensation that I pretended myself to have experienced, but amplified and generalized. But what proves that imitation is not the general rule is that, of the 18 cases which I mention in this group, 11 relate to persons who experienced

sensations quite different from those which I had simulated. Thus I usually pretend that I find out the card by feeling a trembling in my fingers, but I request each person to look out for any sensation that may come. Now there have been subjects who asserted that they felt nothing at all in the fingers, but had experienced a dazzling sensation. It is of course essential at starting to surround the performance of the experiment with a certain stage effect, a certain mystery, which ensures its success.

A young man, who was evidently ill, and who got up in the night in a state of somnambulism, spontaneously declared this trick to be "dangerous," saying that when he had been made to repeat it several times he felt himself strongly affected and was made unwell by it.

SECOND SERIES.

Experiments made with pieces of Metal.

The experiment with the cards may be varied in the following manner. Under this new form it particularly enables us to excite olfactive hallucinations. At least I have hitherto employed it only for this purpose.

Take a certain number of small metal plates of various forms, or pieces of money with different effigies, so as to be able to recognize them afterwards. Then, performing a little comedy which may be infinitely varied according to the ingenuity of the performer, but the foundation of which is always to appear to magnetize these pieces or the person to be experimented upon, one undertakes to recognize the piece touched in one's absence by this person "by virtue of the exquisite delicacy of smell produced by the fluid." On reentering the room, while talking of something else in order to distract the attention of the spectators, one hastens to take up all the pieces one after the other so as to judge which piece has been touched by the difference of temperature, which alone renders it recognizable. This has the advantage over the preceding experiment that an accomplice can be dispensed with. As soon as one has thus naturally fixed upon the touched piece, they are all applied to the nose with a pretence of smelling them to recognize the accusing odour.

The person touching the piece must hold it for a moment between the fingers; by doing this he warms it sufficiently to ensure the success of the experiment. Odour there is none; but if one asserts its existence, and indicates it by any descriptive qualification, one is nearly certain to make the "deceived" persons perceive it when the experiment is repeated upon them.

On the other hand, if it is not specified, they always do it themselves with a remarkable abundance of details, as we have already seen. I have tested this upon 28 persons of both sexes, nineteen of whom, women especially, declared that they perceived a certain odour upon the piece of money which they pointed out, and which had not been specially touched, just as in the experiment of the card.

One young man said that "it seemed" to him that the piece which he had just picked out diffused a "sharp, dry" odour; the second time he recognized this odour more decidedly and became very positive. A lady refused to repeat the experiment because the odour affected her heart, &c.

On one single occasion I excited at the same time the hallucination of a shock and of an odour, by having the piece simultaneously touched and smelt.

Here then we have a certain number of facts which demonstrate how easy it is to induce hallucinations, of vision, of touch, and of smell in persons who are not subject to any thing of the kind, who are in good health, perfectly awake, and appear to be very far from those neurotic conditions to which we shall recur, and which predispose to all the falsifications of sensibility.

As regards hallucinations of hearing I have obtained only a very small number of results, because the arrangements which have just been described are not favourable to their excitement. Nevertheless four persons, prepared in an analogous manner to that described, distinctly heard one of the cards of the figure close to which they placed an ear "diffuse a sort of buzzing sound." Hallucinations of hearing, indeed, are not difficult to excite in a silent medium. In the evening I have often caused people to hear the horn announcing a fire by asserting that I heard it myself; and in a party, if one affirms that one hears the bells ring, one always meets with some persons who assert that they hear them ring also.

It is to be remarked that in all the cases which have just been under consideration, the persuasion of the reality of the sensation is such that it is very difficult to demonstrate to the person that this reality does not exist. It is of no use to tell him that you have deceived him; he is too certain of having experienced the sensation to be willing to agree to it. A lady told me "that she would put her hand in the fire" rather than believe that there was any mistake. The history of the sciences takes cognizance of examples of philosophers who, after having thought that they observed such or such a phenomenon of nature to support a favourite theory, have never been willing to recognize that they have deceived themselves. This is an imperfec-

tion against which we ought to act from the commencement of our studies, and which the professors called upon to direct the first steps of a beginner in the sciences of observation should use every endeavour to correct. Among the persons whom I have succeeded in causing to see and feel what I wished and not the external reality, there were naturalists, physicists, &c., some of them having a name in science. It is certain that we must always severely criticise our own observations, because however able we may be, we do not perfectly know how to make use of our organs, and we are too much inclined to give them our confidence.

The education received in laboratories diminishes the imperfections in question. If beginners in microscopic investigations, for example, have to draw an object previously described and which they have actually before their eyes, they often represent it in accordance with the more or less detailed description of it which has been given to them, and not as it really exists. It is not at all uncommon, in the case of a microscopic preparation, that the beginner declares that he sees such or such a detail of structure of which he has been told, and which the preparation does not show at all. I have ascertained this very frequently. And in this case there is not always deception or falsehood on the part of the pupil, who really sees what the master indicates to him; but he sees it by a sort of hallucination of the psychical centres, and not by a real impression striking his retina. Afterwards in the course of study it is more rare to meet with any such phenomenon in those who devote themselves to observation; but nevertheless it is always necessary to be upon one's guard.

It is also well known that our sensations are not always in relation to the degree of intensity of the external excitation (Werndt, Fechner, &c.). The expectation of a phenomenon reduces to a minimum the degree of intensity of excitation necessary to produce the phenomenon in order that it may be followed by a sensation. "When the expectation is extraordinarily vivid," says Mr. James Sully, "it may suffice to produce as it were the simulacrum of a real sensation. This is what occurs when the existing circumstances suggest to us the idea of some immediate event. The effect is particularly powerful in case the object or event expected is of a nature to interest or excite, because then the mental image gains in intensity by virtue of the emotional excitement which accompanies it"* . These conditions are met with in the facts which we have just related.

* James Sully, *Les illusions des sens et de l'esprit*. A volume of the International Scientific Library, Paris, 1882, p. 78.

Two words more.

In the state of somnambulism, of hysteric or hypnotic neurosis, &c. the human subject may, in consequence of certain practices, be plunged into a peculiar sleep most frequently accompanied by catalepsy. Now during one phase of this sleep (the so-called lethargic phase) the special senses continue in part to act, and by speaking to them the most various hallucinations may be set up in the subjects. If we tell them that music is being played, they hear it immediately; they see the various objects of which we speak to them, they feel them and taste them. These are well-known phenomena of suggestion which have been much studied of late years. In certain invalids the field of suggestion has, so to speak, no limits. It is interesting to find that here the pathological state is only an exaggeration of the normal state, since (as we have just seen) we may by suitable discourses, by persuasion, &c. appeal to the attention of the normal man, not put to sleep, in such a manner as to make him have consciousness of sensations which do not correspond to any objective reality. In both cases the repetition of the hallucination leads to an augmentation of its intensity. All neuropaths know that it is much easier to produce suggestion in old subjects who have been long under the hands of the mesmerists than in those who are put to sleep for the first time. In our experiments we have always found that, after two or three repetitions, the subjective sensation increased in intensity, and that the persons whom one has sustained in the belief in the "fluid" of the so-called magnetized card or piece of money are much better fitted to serve for a demonstration than a person upon whom one has tried the experiment for the first time, and who is not always very positive at the commencement.

To sum up, we demonstrate, under what may be termed an embryonic form, in the healthy human subject, phenomena of suggestion which may acquire great intensity in the diseased subject.

XL. *Note on the Alleged Luminosity of the Magnetic Field.*

By W. F. BARRETT, *Professor of Experimental Physics in the Royal College of Science, Dublin*.*

IT is well known that the late Baron von Reichenbach claimed to have discovered a peculiar luminous emanation arising from the poles of a magnet, resembling a faint

* Communicated by the Author.

electric discharge in rarefied air. This peculiar luminosity was only to be seen in a perfectly darkened room, and even then was only visible to certain persons. Since the publication of Reichenbach's elaborate investigations on this subject numerous attempts have been made by competent observers to see this luminous smoke; but these attempts have generally resulted in failure*; and amid the few cases of success that are recorded (such as by the late Professor Gregory and by Dr. Ashburner) I can find no evidence that proper precautions were taken to avoid the effects of imagination, of deception, or of chance. It is not surprising therefore that the discovery claimed by Reichenbach has been very generally discredited among scientific men in all countries. It has, however, always seemed to me very difficult to explain away the abundant, and in some cases weighty, testimony which Reichenbach adduces—such as the evidence of Professor Endlicher, and others in high social position, who in their normal healthy condition describe these appearances in minute detail, the luminosity they assert springing into existence whenever the magnet was excited, as if a phosphorescent cloud had suddenly been created over the magnetic poles.

Affirmative statements of this kind, however foreign to our present knowledge, are surely worthy of respectful inquiry; and though my own attempts to see the glare have been entirely unsuccessful, I prefer to think some of the necessary conditions of the experiment—such as extreme sensitiveness of the retina—have been absent in my case, rather than conclude from my want of success that the phenomenon has no existence.

Considerations such as these led the recently formed Society for Psychical Research to appoint a Committee to repeat Reichenbach's experiments with the object of testing their accuracy, when a wide range of individuals were examined. As a member of that Committee I have lately been present at a course of experiments, where a remarkable verification was afforded of the fact that, to certain eyes, a faint luminosity accompanies the creation of a powerful magnetic field. The evidence, so far as it goes, seems to me so absolutely unexceptionable that I venture to ask you to place on record a brief statement of the facts so far obtained. The positive evidence afforded by the experiments now to be described cannot be annulled by the fact that on subsequent occasions the trials were, as I am informed, less successful. It is, I

* See, for example, Dr. W. H. Stone's very careful and excellent experiments described in the *St. Thomas's Hospital Reports* (1880), vol. x. p. 100.

think, not unreasonable to conclude that conditions, not yet understood, were sometimes favourable, sometimes the reverse.

The experiments were made in the rooms of the Society, No. 14 Deans Yard, Westminster; one of these rooms was so arranged that it could at pleasure be made into a perfectly dark chamber, no glimmer of light being perceived even after an hour's immersion in the darkness. A powerful electro-magnet was mounted on a heavy wooden stand, and stood by itself in the centre of the room; wires led from the magnet to a commutator in another room, and thence to a large Smee's battery outside. Three observers (Mr. Walter H. Coffin, the Honorary Secretary of this Committee, Mr. Edmund Gurney, and Mr. E. R. Pease) were in charge of the commutator, making and breaking the current at their own pleasure and noting down the exclamations made by the observers in the adjoining darkened room, the voice being easily heard through the intervening curtains. In the dark chamber were Mr. F. W. H. Myers, Dr. A. T. Myers, Mr. H. N. Ridley, and myself, and in addition, on a subsequent occasion, Mr. W. R. Browne, together with two persons who, on a preliminary trial a day or two before, had declared they saw a luminous glare over the poles of a permanent steel magnet. These were Mr. G. A. Smith and a boy, Fred. Wells, who is an assistant in a baker's shop; both of them were entire strangers to these experiments up to the time of our preliminary trials, and disclaimed any knowledge of Reichenbach's work. In the first instance they were not told what to look for, but merely to note if they perceived anything amid the darkness, and if so, what and where.

For some time after entering the dark chamber nothing was seen, though during this time the electromagnet was frequently excited. After about half an hour had elapsed, Wells and subsequently Mr. Smith declared they saw a faintly visible smoke in the room; being asked where, each in turn led me directly up to the magnetic poles as the seat of the luminosity. One pole (the north-seeking pole) they said was brighter than the other. The luminosity was described as like two waving cones of light, with the apex of each cone on the magnetic poles; the breath was able to deflect but not to extinguish the glow*. It was not intercepted they said, by a black velvet cloth nor by a deal board laid flat over the poles,

* So far as I could judge, the appearance must have resembled the long ascending stream of faintly lambent aqueous vapour which is to be seen far above the flame of pure hydrogen, when viewed in a well-darkened room. I have referred to this luminosity in my paper on "Some Physical Effects produced by a Hydrogen-flame," *Phil. Mag.* November 1865.

but they declared *it was at once obscured* when these bodies were held between the eyes of the observers and the magnet, the absolute darkness being of course preserved continuously. When the current was cut off, both the observers simultaneously exclaimed that the light had disappeared.

The current was now at irregular intervals made and broken, by means of the commutator in the next room, and the exclamations of the observers in the dark chamber noted down by those who had charge of the commutator. The commutator worked noiselessly; and no indication whatever was given of the moment when the current was to be put on or taken off. During the experiments Mr. Smith stood near the magnet, touching one of us, and remote from the curtains which separated the dark from the lighter room beyond.

After a few preliminary trials to test the arrangements, a consecutive series of observations extending over an hour was then made by Mr. Smith. From time to time during this period the observers in the next room silently and unexpectedly closed or interrupted the current, the intervals being purposely varied from a few seconds to several minutes. In this way *fourteen* consecutive trials were made; and in every case except one the exclamations made by Mr. Smith, such as "Now I see it," "Now its gone," were absolutely simultaneous with the movement of the commutator—according to the unanimous report of the witnesses in the adjoining room. In the one exception referred to, a delay of five seconds occurred between the breaking of the current and the exclamation: this, however, may easily have been due to a momentary relaxation of attention on the part of Mr. Smith. The strain on the attention was indeed so severe, that after the fourteenth observation Mr. Smith complained of considerable pain in his eyes and head and was obviously much exhausted. During a succeeding half hour two or three further experiments were made; but the results were uncertain, and may, I think, be fairly excluded. It may be noted that Mr. Smith and Wells did not at any time appear to have unusual powers of vision for the objects in the darkened room.

It is obvious that a series of accidental coincidences between the act of closing or opening of the circuit and the exclamation of the observer cannot explain the facts here noted. As there are 3600 seconds in an hour, to hit off any one right moment by pure chance would be very improbable; but the chances against success increase in geometric progression when 14 right moments are *successively* hit off. The probabilities against mere coincidence as an explanation are therefore many millions to one.

More important was the possibility of indications being afforded by the act of magnetization and demagnetization, which might give notice to the observer and suggest to the imagination the conversion of an illusion into a fancied reality.

Of these indications the so-called "magnetic tick" at once suggested itself. Knowing precisely what to listen for, and therefore more keenly alive to the sound than Mr. Smith, who presumably knew nothing of this molecular crepitation, I failed to detect the faintest sound on the "making" of the circuit; and a barely audible tick on "breaking" contact was heard only when my ear was in close contact with the magnet or its support. This was due to the massive character of the magnet and stand, which also prevented any other discernible movement when the magnet was excited. Further, I satisfied myself that, at the distance at which Mr. Smith stood from the magnet, it was impossible to discover when the circuit was completed or interrupted by the attraction of any magnetic substance about one's body; as a precaution, however, Mr. Smith emptied his pockets beforehand. At the same time it is quite possible a skilful operator, bent upon deceiving us, might be able to detect the moment of magnetization and demagnetization by feeling the movement of a concealed compass-needle. Against this hypothesis must be placed the fact that no information was given to Mr. Smith beforehand of the nature of the experiment; and he had no object to serve by professing to see what he really did not see. Ultimately all scientific observation rests upon the good faith of the observers; and there was nothing to arouse the smallest suspicion of the good faith of the observer in the present instance.

Similar experiments were made on another evening with the boy Wells, with fairly satisfactory results. In the case of Wells the luminosity, from his description, must have appeared to be brighter and larger; and on the interruption of the circuit it was not instantly extinguished, but rapidly died away*; his frequent exclamation on breaking the current was "Oh, you are spoiling it."

Wells was also tried in the dark chamber with two permanent horseshoe magnets, and saw the luminosity clearly on both. Unknown to Wells, I silently changed the position of the two magnets; he at once detected where they were placed. Holding one of the magnets in my hand, Wells told me correctly whether I moved the magnet up or down or held it stationary; this was repeatedly tried with success. In this case the poles

* There was a considerable amount of residual magnetism in the electromagnet.

of the horseshoe were very close together, so that there was a small intense magnetic field; from the juxtaposition of the poles no effect could be produced on a small compass-needle at one tenth of the distance at which I ascertained Wells actually stood—supposing, which is highly improbable, that the lad had the intention to deceive and knew how to attempt it.

Numerous questions of interest suggest themselves, such as the photographic and prismatic examination of the luminosity and whether the light is polarized or capable of being polarized, or whether the rarefaction and removal of the air around the poles affects the luminosity. The answer to these and cognate questions, together with the examination of some remarkable collateral phenomena that presented themselves—such as the variation of the intensity of the light when viewed in different azimuths, or along or across the magnetic axis, and the effect of certain bodies on the light—will become the subject of investigation by the Committee whenever the testimony to the simple fact itself has been sufficiently well established by various observers. The object of the present note is merely to demonstrate that there is a strong *primâ facie* case in favour of the existence of some peculiar and unexplained luminosity, resembling phosphorescence, excited in the region of the atmosphere immediately around the magnetic poles, and which can only be seen by certain individuals.

XLI. On Self-Regulating Dynamo-electric Machines.

By R. H. M. BOSANQUET*.

DYNAMO-ELECTRIC machines consist mainly of two parts—the armature and the electromagnets; and the principle associated with the use of the term dynamo is, that the current produced by the armature is, wholly or in part, used to excite the electromagnets. Thus dynamo-electric machines are to be distinguished from magneto-electric machines, in which permanent magnets are used.

Simple dynamo-electric machines are of two kinds—main-circuit machines and shunt-circuit machines. In main-circuit machines the whole of the main current is sent round the electromagnets; in shunt-circuit machines a branch current is diverted from the main current, and used to excite the electromagnets.

Both of these types fail under certain conditions of the

* Communicated by the Author, being a Paper read before the Ashmolean Society, March 5, 1883.

external resistance ; and the conditions of failure are opposite in the two cases.

Main-circuit machines fail when the external resistance is high ; for then the current produced is small, the excitation of the magnets fails, and the action ceases.

Shunt-circuit machines fail when the external resistance is low, as when the carbons of an arc lamp accidentally touch, and the resistance becomes very small ; for then the whole current is diverted from the shunt to flow through the short circuit, the excitation of the shunt magnets fails, and the action ceases.

It has been proposed by several persons, independently, to get rid of these failures by combining the two modes of excitation. For this purpose the magnets are wound both with main and shunt coils—the main coils carrying the main circuit, and the shunt coils carrying a derived circuit. The main coils are most strongly excited when the shunt coils fail ; and the shunt coils are most strongly excited when the main coils fail. Machines wound with both main and shunt coils have been called compound machines, as opposed to the original simple types with main coils only or shunt coils only.

Compound machines are capable of being arranged so as to fulfil certain conditions—for example, to give a constant E.M.F. for varying values of the external resistance. When arranged to satisfy any such requirement, they have been called self-regulating machines.

Several types of self-regulating machines are in the market ; but their principles have not been made public.

The first publication on the theory of this matter appears to have been by Marcel Desprez, in '*La Lumière électrique* ;' this publication is not accessible here. From the accounts that have been given of it, it does not appear to treat the matter from my point of view.

The first publication on the theory in England, so far as I know, was in a letter I wrote to the '*Electrician*' in December 1882, in which I gave the deduction of the form of compound machines to produce constant E.M.F. at constant velocity, based on the simplest theoretical assumptions.

This letter produced numerous communications from inventors claiming the priority in such machines—more especially a general description by Mr. Paget Higgs, and a series of papers by Mr. Gisbert Kapp, in one of which a graphical method was described for the examination by trial of different forms of winding, no doubt a satisfactory means of arriving at practical results. Attention has also been called to the matter in various public lectures. These have been partly

descriptive, and partly devoted to the introduction of new and ingenious arrangements. But, so far as can be judged from the reports available, nothing has been added to the theory, regarded from my point of view.

I have not been able to experiment largely on the subject, as it would necessitate the abandonment of other work; and there are other laboratories specially instituted for electrical work, where this may more properly be done. But I have made one successful experiment which is worth mentioning.

My Gramme machine fails, as all main-circuit machines fail, when it is attempted to drive a current through a high external resistance. With any thing over 10 ohms the machine fails. Now it was obvious that a comparatively small shunt coil wound over the main coils would be well excited in the case of high external resistance, and would assist to start the action of the machine in these cases.

The space available round the main coils was small; but I succeeded in winding nearly 2000 coils of a small wire on pasteboard cylinders which fit over the main coils. The resistance of the new coils is about 27 ohms. They are attached to the armature-brushes, and form a shunt. They make a great difference in the performance of the machine.

Without joining up the main circuit at all, it is possible to obtain an E.M.F. of 70 or 80 volts with the shunt circuit alone.

This is sufficient for all ordinary purposes, and is particularly useful in charging accumulators; for it obviates the necessity of all the troublesome arrangements for starting the charging which I have previously described to the Society*, besides obviating the chance of reversal so long as a reasonable amount of power is kept in action.

It is no small experimental result that any Gramme machine can have its capacity for work so greatly increased at the small cost of time and material necessary to add to it shunt coils of this description.

The velocity and conditions of motion of a dynamo driven by a prime mover depend on the equality of the power supplied by the prime mover and the power absorbed by the machine. We have therefore, first, to examine the nature of the relation between the power supplied and the velocity, and, secondly, the relation between the power absorbed and the velocity.

The relation between the power supplied by a prime mover and the velocity is determined generally by a governor. Whether this be the case or not, we can suppose the powers

* *Phil. Mag.* xiv. p. 250.

set off as ordinates p , and the velocities as abscissæ v . We shall then obtain a curve corresponding to a relation between p and v , which we may call the governing function of the prime mover or engine.

When the load is very great it will bring the engine to rest; then the velocity and power supplied are both zero. Again, when the engine is running free from load it is supplying no work, and the velocity has its greatest value.

The most general expression satisfying these conditions is based upon the form

$$p = P \sin \frac{v}{V} \pi,$$

where P is the maximum power absorbed, V velocity of free running.

This law is illustrated in fig. 1.

By Fourier's theorem any law whatever of this kind

may be represented by an equation of the form

$$p = P_1 \sin \frac{v}{V} \pi + P_2 \sin 2 \frac{v}{V} \pi + P_3 \sin 3 \frac{v}{V} \pi \\ + \dots \dots \dots \text{ad infinitum};$$

where P_1 , P_2 , &c. are coefficients, positive or negative, to be determined in accordance with the law to be represented. The sharper the point of the curve, the more considerable the higher terms of the series will be.

These forms, however, are useless for simple purposes; and we have to consider the representation of governing relations by simple approximate formulæ.

The simplest assumption for many purposes is that the curve consists of two straight lines, forming a triangle whose base is V , the velocity of free running. The altitude of the vertex is P , the maximum power expended. If we suppose the vertex a little rounded, this may be adjusted to represent many cases.

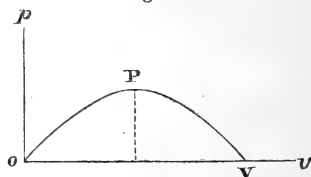
Along OP the law may be written $p = av$.

Along PV it may be written $p = -\beta(v - V)$.

The latter assumption is not convenient for our present purpose; and we shall prefer to represent

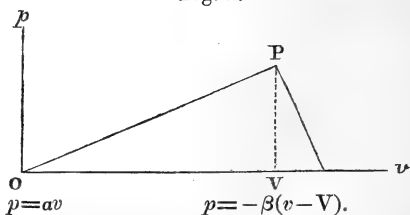
the falling part of the curve, which shows the cutting-off of the

Fig. 1.



$$p = P \sin \frac{v}{V} \pi.$$

Fig. 2.



power at increasing speeds by the governor or otherwise, by a hyperbolic curve.

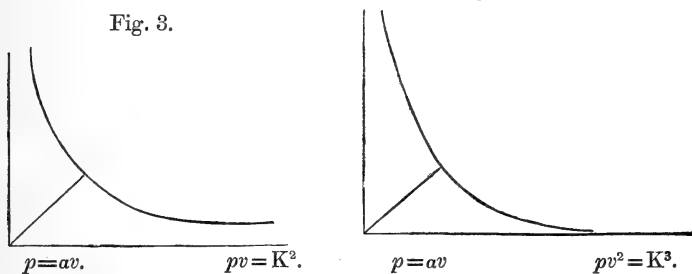
(1) Common rectangular hyperbola $p v = K^2$ (fig. 3).

(2) $p v^2 = K^3$ (fig. 4).

(n) $p v^n = K^{n+1}$.

Fig. 4.

Fig. 3.



(1) $\frac{dp}{dv} = -\frac{K^2}{v^2},$

\therefore at the vertex, where $p = v = K, = -1$;

and at this point, on the bisector of the axes, the curve is inclined downwards at 45° .

(2) $\frac{dp}{dv} = -2\frac{K^3}{v^3} = -2$ at vertex ;

\therefore curve at vertex is inclined downwards at $63\frac{1}{2}^\circ$.

Similarly the inclination downwards at the vertex increases rapidly ; and for

(n) $\frac{dp}{dv} = -n\frac{K^{n+1}}{v^{n+1}}$

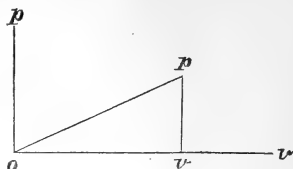
the tangent of the downward-pointing angle is n .

We can approximately represent most governing relations by a straight line meeting such a hyperbolic curve. The part which represents the state of things where the governor is in action is on the curve beyond the point of junction with the straight line.

Consider first the straight line from 0; $p = a v$. The power is proportional to the velocity. This will be the case so long as the steam is supplied at constant pressure. For power is pressure on piston \times velocity. But pressure on piston is constant; \therefore power varies as velocity. The hyperbolic curves represent governors which are sharper and sharper as n increases. In the case of an infinitely sharp governor the governing function, or curve of supplied power,

is a vertical straight line. The actual value of p is in this case indeterminate; it may vary from zero up to the full height of the straight line vp (fig. 5); and the smallest variations of v will cause it to oscillate between these extreme values. Uniform motion would not be practically possible under an arrangement of this kind, and it does not answer for the governor to be too sharp. A moderate inclination represents the case of all practical governors which depend on variation of velocity*.

Fig. 5.



These remarks do not apply to phase-governors such as that of my uniform-rotation machine. In this machine the average velocity is always that due to the connexion with the clock; and the governing action is produced by variations of the distance at which the machine follows the clock, so to speak. I believe that governors of this type are in the market. In these cases the power delivered is determined by considerations other than the variation of velocity.

If in such cases it is desired to impose conditions on the supply of power, the phase, or distance of following the standard motion, must be used to control the governing action, instead of any mechanism depending on absolute changes of velocity.

We have to deal, secondly, with the relation which represents the power absorbed by the machine at different velocities.

Consider the case of machines with constant excitation—including magneto machines, machines with magnets separately excited, and dynamo machines in which the magnets are saturated. Let \mathfrak{B} be the constant field intensity at right angles to the circumference of the armature, v the velocity, l the effective length of the armature-wire, and E the E.M.F. developed. Then

$$E = \mathfrak{B}vl \text{ in C.G.S. measure,}$$

power absorbed

$$p = \frac{E^2}{R + r}$$

* It has been said that gas-engine governors are of the infinitely sharp type; but this is not so. When the impulses are discontinuous and at considerable intervals, the effect of their occasional suppression is distributed in the same way as their normal action. The effect of a governor which shut off the steam from an ordinary engine completely on the least rise of speed would be very different.

(R resistance of armature, r remaining resistance),

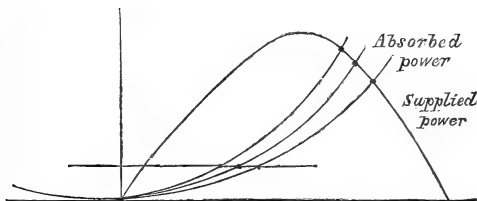
and

$$p = \frac{\mathfrak{B}^2 v^2 l^2}{R + r},$$

r being constant; this is a parabola. As r varies, the parameter of the parabola varies, and we have a series of curves such as those shown in fig. 6.

Now draw any governing curve, or curve of supplied power (fig. 6); it intersects the curves of absorbed power in points

Fig. 6.



which represent equality between supplied and absorbed power, and therefore defines the motion of the machine for the values of r concerned.

If the governing curve be regarded as the assemblage of such points, it defines the motion of the machine under the action of a given engine as the resistance varies.

We may invert and generalize the process.

Take the function of absorbed power $f(p.v.r)=0$. It is required to define the motion so as to satisfy a given condition.

Between $f(p.v.r)=0$ and the given condition eliminate r . There results an equation of the form $F(p.v)=0$, which must hold for all points defining the motion. This, if embodied in a governor, will determine the motion so as to satisfy the condition independently of r .

There is one general condition which must be satisfied that the motion may be stable. This is, the curve of absorbed power must pass from below to above the curve of supplied power at the intersection as v increases.

For in this case, if the velocity be accidentally increased, more power is absorbed than supplied, and the velocity is checked. Similarly, if the velocity be accidentally diminished, more power is supplied than absorbed, and the velocity is increased and the motion is stable.

If the curves coincided, the equilibrium of the motion would be neutral.

If the curve of absorbed power passed from above to

below the curve of supplied power with increasing v , the motion would be unstable.

The condition of stability may be thus expressed: if p_a be power absorbed and p_s power supplied, $\frac{dp_a}{dv} - \frac{dp_s}{dv}$ must be positive.

I will now apply the above rules to the case already mentioned of a constant field \mathfrak{B} normal to the armature surface; this includes magneto machines, machines with separate excitation, and saturated magnets.

$$(1) \text{ The equation of the machine is } p = \frac{\mathfrak{B}^2 v^2 l^2}{R + r}.$$

(2) Let the condition be $E = \text{constant}$.

$$\therefore v = \text{constant, since } E = \mathfrak{B}vl.$$

This is already independent of r ; so it is not necessary to eliminate. And $v = \text{constant}$ is the ideal governor. It cannot be realized by a function of v and p .

(3) Regarded as a line of great steepness directed downwards, this ($v = \text{const.}$) satisfies the condition of stable motion; but if the line be absolutely upright, the equilibrium is neutral; for if it were turned over in the least, the equilibrium would be unstable, except for the greatest value of p .

I will now take the same case subject to the condition of constant current:—

$$(1) \text{ is } p = \frac{\mathfrak{B}^2 v^2 l^2}{R + r}, \text{ as before.}$$

$$(2) C = \text{const.},$$

$$R + r = \frac{E}{C} = \frac{\mathfrak{B}vl}{C};$$

$$\therefore p = C\mathfrak{B}vl, \text{ the required governing function.}$$

$$(3) p = C\mathfrak{B}vl \text{ is capable of governing } p = \frac{\mathfrak{B}^2 v^2 l^2}{R + r} \text{ in stable motion.}$$

A governing arrangement of the type $p = kv$ can be realized, as observed above. It is only necessary to supply steam at constant pressure, by means of a reducing-valve or otherwise. Then the power is pressure \times velocity, and is proportional to the velocity when the pressure is constant.

We hear of self-regulating arrangements of dynamo machines by which the current is supposed to be kept constant under varying resistance. But since the electrical power absorbed is $C^2(R + r)$, it is obvious that some arrangement must be made to supply a power proportional to the resist-

ance. And this must ultimately depend on the governing function of the prime mover.

The following example leads to an unstable solution.

Same machine with constant field = \mathfrak{B} . It is required to keep the difference of potential between the armature-brushes constant. This includes the case of constant potential between the ends of leads.

R is the resistance of armature (with leads),
 r external resistance;

$e = \frac{r}{R+r} E$ is to be kept constant.

$$(1) \quad p_a = \frac{\mathfrak{B}v^2 l^2}{R+r}.$$

$$(2) \quad e = \text{const.} = \frac{r}{R+r} E = \frac{r}{R+r} \mathfrak{B}vl,$$

$$r = \frac{Re}{(\mathfrak{B}vl - e)},$$

$$R+r = R \frac{\mathfrak{B}vl}{\mathfrak{B}vl - e},$$

$$p_s = \frac{\mathfrak{B}vl}{R} (\mathfrak{B}vl - e).$$

$$(3) \quad \frac{dp_a}{dv} - \frac{dp_s}{dv} = - \frac{2rv\mathfrak{B}^2 l^2}{R(R+r)} + \frac{\mathfrak{B}le}{R}$$

$$= \frac{\mathfrak{B}l}{R} \left(- \frac{2rE}{R+r} + e \right)$$

$$\left(\text{since } v = \frac{E}{\mathfrak{B}l} \right)$$

$$= - \frac{\mathfrak{B}le}{R};$$

and the governing function which satisfies the required condition gives rise to unstable motion.

I will now consider the simplest assumption that can be made in connexion with the ordinary main-circuit machine. It is that, according to the law of electromagnets generally accepted, the excitation of the magnets is proportional to the number of coils and to the current circulating in them, or

$$\mathfrak{B} = knC;$$

nC may be called current-turns.

The equation $E = \mathfrak{B}vl$ then becomes

$$E = knCvl.$$

In main-circuit machines, $C = \frac{E}{R+r}$,

$$\therefore 1 = \frac{knvl}{R+r},$$

and v can only have one value for a given value of r , which does not correspond with fact.

The same difficulty applies to dynamo machines of all types. The inference is that the assumption that the field-magnetism is proportional to the number of current-turns is wrong.

In fact, in the equation $E = \mathfrak{B}vl$, if E is not to strike out on both sides, \mathfrak{B} must be some other function of E than the simple first power. The point is most easily dealt with by assuming \mathfrak{B} proportional to a power γ of the current-turns. γ may not be an absolute constant; but its value can be easily obtained in the different parts of the range of the machine.

Then we get an equation of the form

$$E = K_1 E^\gamma v,$$

or

$$E^{1-\gamma} = K_1 v$$

for given resistance.

Dividing by $(R+r)^{1-\gamma}$

$$C^{1-\gamma} = K_2 v,$$

$$C = K_3 v^{\frac{1}{1-\gamma}}.$$

Put

$$\frac{1}{1-\gamma} = x, \text{ or } 1-\gamma = \frac{1}{x}, \text{ or } \gamma = 1 - \frac{1}{x};$$

then

$$C = K_3 v^x,$$

a relation which can be easily examined experimentally.

I have examined this relation with my Gramme machine, chiefly using the main circuit only. It is clear that the excitation by a given number of current-turns cannot depend on their supply by shunt or main circuit; so that it is sufficient to examine the law in this simpler case.

The result may be described by assigning approximate values. I consider 5 to 20 ampères the practical range of my machine. Without pretence to accuracy, I obtain

Current. ampères.	x .	γ .
5	3	$\frac{2}{3}$
10	2	$\frac{1}{2}$
15 } 20 }	average 1.25	$\frac{1}{5}$

$x=1, \gamma=0$ correspond to the condition of saturation.

Theoretical Basis.

I will now examine the effect of a small change of velocity dv in a dynamo, first on the ordinary theory, and then according to the hypothesis above indicated.

Let the equation of development of E be

$$E_d = KNv = K'E_v, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and suppose that initially $E_d = E = E_0$,

v becomes $v + dv$.

An increase takes place in the E developed ;

$$d_1 E = K'E_0 dv,$$

or, dividing by (1),

$$\frac{d_1 E}{E_0} = \frac{dv}{v}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This increase develops in time an increase of magnetism, so that E on the right of (1) becomes

$$= E_0 + d_1 E.$$

The increased magnetism develops in turn on the left an increase of E , $= d_2 E$; and the amount of the change is the same as in the first case; and so on.

Hence there are an infinite number of equal increments of E.M.F. developed by a single change dv ; and the total change may be expressed,

$$\frac{\Delta E}{E} = \infty \frac{dv}{v}.$$

This being contrary to experience, we will introduce the hypothesis that the change of magnetism falls short of the change of exciting current in a ratio γ , which is less than unity.

We arrive as above at equation (2),

$$\frac{d_1 E}{E_0} = \frac{dv}{v}.$$

An increase of magnetism is next developed; but by hypothesis, instead of being the full increase due to $d_1 E$, it is dimi-

nished in ratio γ . So that the next increment of E , developed by the motion in the increased field, is defined by

$$\frac{d_2 E}{E_0} = \gamma \frac{dv}{v}.$$

Similarly each successive term is derived from the preceding by multiplication by γ . Whence

$$\begin{aligned} \frac{\Delta E}{E} &= (1 + \gamma + \gamma^2 + \dots) \frac{dv}{v} \\ &= \frac{1}{1 - \gamma} \frac{dv}{v}, \end{aligned}$$

or, if we now write dE for ΔE , since the total change is comparable with dv ,

$$\frac{dE}{E} = \frac{1}{1 - \gamma} \frac{dv}{v}.$$

Integrating,

$$\log_e v = (1 - \gamma) \log E + \text{constant},$$

or

$$v = E^{1 - \gamma} \times \text{constant};$$

which is the form of equation above assumed.

We will now consider the general case of a machine having the magnets wound both with main and shunt coils.

Number of Coils.

n main-magnet coils. ν shunt coils.

Resistances.

R armature, r external and magnets, ρ shunt coils.

r resultant of r and ρ in parallel circuit.

$$\frac{1}{r} = \frac{1}{r} + \frac{1}{\rho}.$$

It is required to simplify $\frac{r}{R + r}$.

$$\begin{aligned} r &= \frac{1}{\frac{1}{r} + \frac{1}{\rho}} \\ &= \frac{r}{1 + \frac{r}{\rho}}; \end{aligned}$$

$$\begin{aligned}
 \frac{r}{R+r} &= \frac{\frac{r}{1+\frac{r}{\rho}}}{R+\frac{r}{1+\frac{r}{\rho}}} = \frac{r}{R\left(1+\frac{r}{\rho}\right)+r} \\
 &= \frac{r}{R+r\left(1+\frac{R}{\rho}\right)} \\
 &= \frac{r}{R+rs},
 \end{aligned}$$

where $s=1+\frac{R}{\rho}$, and is a little greater than unity.

E.M.F.

E = total E.M.F., e between armature terminals.

$$\begin{aligned}
 e &= \frac{r}{R+r} E \\
 &= \frac{r}{R+rs} E.
 \end{aligned}$$

Current.

$$\begin{aligned}
 C \text{ in main coils} &= \frac{e}{r} \\
 &= \frac{E}{r} \frac{r}{R+r} \\
 &= \frac{E}{R+rs};
 \end{aligned}$$

$$\begin{aligned}
 c \text{ in shunt coils} &= \frac{e}{\rho} \\
 &= \frac{r}{\rho} \frac{E}{R+rs}.
 \end{aligned}$$

Current-turns = N .

$$nC + \nu c = N,$$

$$\begin{aligned}
 \left(n + \frac{\nu}{\rho} r\right) \frac{E}{R+rs} &= N \\
 &= FE,
 \end{aligned}$$

where

$$F = \frac{n + \frac{\nu}{\rho} r}{R + rs}.$$

Magnetic field = \mathfrak{B} .

$$\mathfrak{B} = \lambda N^\gamma$$

$$= \lambda (FE)^\gamma.$$

Equation of E.M.F. developed.

$$E = \mathfrak{B}vl.$$

v = velocity of conductor of armature at right angles to lines of force,

l = effective length of conductor;

$$\therefore E = l\lambda v (FE)^\gamma,$$

$$E^{1-\gamma} = l\lambda v F^\gamma,$$

$$E = (l\lambda v)^{\frac{1}{1-\gamma}} F^{\frac{\gamma}{1-\gamma}}.$$

Put

$$\gamma = 1 - \frac{1}{x}, \quad 1 - \gamma = \frac{1}{x}, \quad \frac{1}{1-\gamma} = x;$$

$$E = (l\lambda v)^x F^{x-1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (I.)$$

where

$$F = \frac{n + \frac{\nu}{\rho} r}{R + rs}, \quad s = 1 + \frac{R}{\rho}.$$

For main circuit only, $\rho = \infty$, $\nu = 0$, $s = 1$.

$$F = \frac{n}{R + r}.$$

For shunt circuit only, $n = 0$.

$$F = \frac{\nu}{\rho} \frac{r}{R + rs}.$$

$\frac{\nu}{\rho}$ determines the wire of the shunt. It is the number of turns that has a resistance of 1 ohm. Otherwise, $\frac{\rho}{\nu}$ is the resistance of one turn of the shunt coil.

Dynamo with constant E.M.F. and Velocity.

If $r=0$, or the main circuit is short-circuited,

$$F = \frac{n}{R}.$$

If $r=\infty$, or the main circuit is not joined up,

$$F = \frac{\nu}{\rho s} = \frac{\nu}{R + \rho}.$$

If E and v are to be the same in all cases, they must be the same in the extreme cases when the main circuit is short-circuited and not joined up. In fact, by (I.), F is constant if x is constant. Whence, equating the above values,

$$\frac{n}{R} = \frac{\nu}{R + \rho},$$

which is the condition for machines of constant E.M.F. and velocity, under circumstances for which x can be considered constant.

The same condition can be obtained more generally by making F constant for all values of r .

$$F(R + rs) = n + \frac{\nu}{\rho} r.$$

If this is true independently of r , the coefficient of r and the constant terms must vanish separately;

$$\therefore FR = n, \quad F = \frac{\nu}{R + \rho},$$

and

$$F = \frac{n}{R} = \frac{\nu}{R + \rho},$$

as before. This only holds true so long as x is constant.

This condition was first given in my letter to the 'Electrician,' but without having regard to s , or to the variation of x , which is a much more serious thing, or to the stability, which is more serious still.

In order to take count of these matters, we must prepare the general values of E , C , and p .

From (I.) we have

$$E = (l\lambda v)^{\frac{1}{2}} \left(\frac{n + \frac{\nu}{\rho} r}{R + rs} \right)^{\frac{1}{2}}.$$

Hence

$$C = \frac{E}{R+rs} = \left(n + \frac{\nu}{\rho} r\right)^{x-1} \left(\frac{l\lambda\nu}{R+rs}\right)^x,$$

$$p = \frac{E^2}{R+r}, \quad \frac{1}{R+r} = \frac{1}{R + \frac{r}{1 + \frac{r}{\rho}}} = \frac{1 + \frac{r}{\rho}}{R\left(1 + \frac{r}{\rho}\right) + r}$$

$$= \frac{1 + \frac{r}{\rho}}{R+rs},$$

$$p = (l\lambda\nu)^{2x} \left(\frac{n + \frac{\nu}{\rho} r}{R+rs}\right)^{2(x-1)} \frac{1 + \frac{r}{\rho}}{R+rs}$$

$$= (l\lambda\nu)^{2x} \frac{\left(n + \frac{\nu}{\rho} r\right)^{2(x-1)}}{(R+rs)^{2x-1}} \left(1 + \frac{r}{\rho}\right).$$

I will now assume the condition $E = \text{constant}$. We have to eliminate r between this and the equation for p . The constants of the machine will then be at disposal for determining the governing relation.

(1) We have

$$p = E^2 \frac{1 + \frac{r}{\rho}}{R+rs},$$

$$p(R+rs) = E^2 \left(1 + \frac{r}{\rho}\right),$$

$$pR - E^2 = r \left(\frac{E^2}{\rho} - ps\right).$$

(2) Again,

$$\frac{E^{\frac{1}{x-1}}}{(l\lambda\nu)^{\frac{x}{x-1}}} = K \text{ say} = \frac{n + \frac{\nu}{\rho} r}{R+rs},$$

$$K(R+rs) = n + \frac{\nu}{\rho} r,$$

$$KR - n = r \left(\frac{\nu}{\rho} - Ks\right).$$

Eliminating r ,

$$\frac{pR - E^2}{KR - n} = \frac{\frac{E^2}{\rho} - p_s}{\frac{\nu}{\rho} - K_s},$$

whence

$$p \left(\frac{R\nu}{\rho} - sn \right) = \frac{E^2}{\rho} (\nu - n - K\rho).$$

The condition $\frac{n}{R} = \frac{\nu}{\rho s}$ makes the coefficient of p vanish; so that the power supplied must be independent of the velocity. We have seen that in a similar case the equilibrium of the motion is neutral, and the arrangement is not likely to be of use.

The value of ν in this case will depend on E and on x , and is determined by the condition

$$\nu - n - K\rho = 0.$$

(3) It remains to examine the stability. Writing for K its value, the governing relation becomes

$$p_s \left(\frac{R\nu}{\rho} - sn \right) = \frac{E^2}{\rho} \left(\nu - n - \frac{E^{\frac{1}{x-1}}}{(\lambda\nu)^{\frac{x}{x-1}}} \rho \right).$$

If we assume $\frac{\nu}{\varepsilon\rho} > \frac{n}{R}$, the motion will not generally be stable. For, using the general value of the power absorbed,

$$\frac{dp_a}{dv} = 2x(\lambda)^{2x} \left(\frac{\nu}{R + rs} \right)^{2x-1} \left(n + \frac{\nu}{\rho} r \right)^{2(x-1)} \left(1 + \frac{r}{\rho} \right),$$

and

$$\frac{dp_s}{dv} = \frac{E^2}{\left(\frac{R\nu}{\rho} - sn \right)} \cdot \frac{x}{x-1} \frac{E^{\frac{1}{x-1}}}{(\lambda)^{\frac{x}{x-1}}} \cdot \frac{1}{\nu^{\frac{1}{x-1}}}.$$

Both terms of $\frac{dp_a}{dv} - \frac{dp_s}{dv}$ are therefore positive in themselves.

As x comes to differ little from 1 the second term increases indefinitely; therefore anywhere near the point of saturation of the magnets the motion would be unstable.

We must therefore have

$$\frac{\nu}{s\rho} < \frac{n}{R}.$$

Then $\frac{dp_s}{dv}$ becomes negative in itself, and $\frac{dp_a}{dv} - \frac{dp_s}{dv}$ is always positive, so that the motion is stable.

The governing relation is therefore

$$p = \frac{E^2}{spn - Rv} \left(n - v + \frac{E^{\frac{1}{x-1}}}{(l\lambda v)^{\frac{x}{x-1}}} \rho \right).$$

The practical deductions are as follows:—The ratio of shunt and main coils is subject to a condition in order that the required governing relation for constant E.M.F. may be stable. This condition may be put in the form

$$\frac{R}{R + \rho} < \frac{n}{v}.$$

This tells us therefore that the ratio of the number of main coils to shunt must be greater than that of armature resistance to sum of armature and shunt resistance.

A suitable value of R/ρ might be 1/100. Then, suppose there are 10,000 coils in the shunt, there must be more than 101 coils of the main circuit.

Under these circumstances it would appear that there is another condition to be fulfilled in order that the expression for the power may be positive, viz.

$$\frac{v - n}{\rho} < \frac{E^{\frac{1}{x-1}}}{(l\lambda v)^{\frac{x}{x-1}}}.$$

But, remembering that $\frac{E}{(l\lambda v)^x} = F^{x-1}$, this condition becomes

$$\frac{v - n}{\rho} < \frac{n + \frac{v}{\rho} r}{R + rs};$$

and it can be shown that this reduces to the same condition as that found above, viz.

$$\frac{R}{s\rho} \left(\text{or } \frac{R}{R + \rho} \right) < \frac{n}{v}.$$

So that it is only necessary to satisfy this one condition.

This being so, the governing function assumes the form

$$p = -A + \frac{B}{v^{\frac{x}{x-1}}}.$$

A could be got rid of by making $n = \nu$, which satisfies the conditions. Then there remains a hyperbolic governor, $pv^{\frac{x}{x-1}} = B$. This does not, however, appear to be a good solution in practice. One would be disposed to have ν at least three times as great as n . In my Gramme machine as I have rewound it, $R/R + \rho$ is about $1/55$, n/ν about $1/2$: this is well within the conditions, and yet past the point $\nu = n$. For practical purposes the shunt excitation would bear to be much stronger. It is difficult, however, to deal with practical applications at present, as we know nothing definite about the governing functions of the engines.

As x approaches unity the sharpness of the governor required is much increased. This seems to point to a control of the governor by the current; for this increases as x approaches 1, or as the magnets approach the point of saturation.

An interesting result is obtained in the case of main coils only, where E is to be constant.

Putting $\rho = \infty$, $\nu = 0$, $s = 1$, in the general formulæ,

$$E = \left(\frac{n}{R+r} \right)^{x-1} (l\lambda v)^x, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$p = \frac{E^2}{R+r}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

from (1),

$$R+r = n \frac{(l\lambda v)^{\frac{x}{x-1}}}{E^{\frac{1}{x-1}}};$$

whence from (2),

$$p = \frac{E^{\frac{2x-1}{x-1}}}{n(l\lambda v)^{\frac{x}{x-1}}}.$$

For $x = 2$, this is of the form

$$p = \frac{A}{v^2},$$

a hyperbolic governor of moderate sharpness.

$$\text{If } x = 3, \quad p = \frac{B}{v^{\frac{3}{2}}},$$

less sharp. If x diminish nearly to 1, the sharpness required is greatly increased.

These cases are interesting, since they explain the possible
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bility of a self-regulation by main coils alone, which was pointed out experimentally by me in a former paper (Phil. Mag. xiv. p. 247).

General case of constant current :

$$p = \frac{E^2}{R+r} = \frac{E^2}{R+rs} \left(1 + \frac{r}{\rho}\right),$$

and

$$E = C(R+rs);$$

$$\therefore p = C^2(R+rs) \left(1 + \frac{r}{\rho}\right);$$

condition, $c = \text{constant}$,

$$C = \left(n + \frac{\nu}{\rho} r\right)^{x-1} \left(\frac{l\lambda v}{R+rs}\right)^x.$$

The general elimination of r is hardly possible.

But taking main coils only,

$$\rho = \infty, \quad \nu = 0, \quad s = 1,$$

$$p = C^2(R+r),$$

$$C = n^{x-1} \left(\frac{l\lambda v}{R+r}\right)^x;$$

or

$$C^{\frac{1}{x}} = n^{\frac{x-1}{x}} \frac{l\lambda v}{R+r},$$

$$p C^{\frac{1}{x}} = C^2 n^{\frac{x-1}{x}} l\lambda v,$$

$$p = C^{\frac{2x-1}{x}} n^{\frac{x-1}{x}} l\lambda v,$$

a rectilinear governor as before, which can govern $p = Ar^{2x}$ in stable motion.

There can be no doubt that the case of constant current is best dealt with by main coils only.

The governing function supplying power proportional to the velocity is probably the only example of a simple theoretical governing function which could be easily realized in a rigorous manner in practice. This would be done, as has been observed already, by a reducing valve, delivering steam in unlimited quantity at constant pressure.

I will now apply the method up to a certain point to a particular case of the general problem, in which the E.M.F. between armature-brushes is required to be constant.

$$\frac{rE}{R+rs} = e = \text{constant}.$$

From

$$p = E^2 \frac{1 + \frac{r}{\rho}}{R + rs}$$

and the above we get a quadratic in r ,

$$r^2 \left(p^2 - e^2 \frac{s}{\rho} \right) - r e^2 \left(2 \frac{R}{\rho} + 1 \right) - e^2 R = 0.$$

Then from

$$E = \frac{R + rs}{r} e = (\lambda v)^x \left(n + \frac{\nu}{\rho} r \right)^{x-1}$$

we have

$$e = r \left(\frac{\lambda v}{R + rs} \right)^x \left(n + \frac{\nu}{\rho} r \right)^{x-1}.$$

In the general case this is intractable; but for particular values of x something can be made of it.

Let $x=2$, which corresponds to a wide range somewhat below the point of saturation of the magnets. Then we get another quadratic in r ,

$$r^2 \left(s^2 e - \frac{\nu}{\rho} (\lambda v)^2 \right) + r (2eRs - n(\lambda v)^2) + eR^2 = 0.$$

The next step is to eliminate r between the two quadratics. I have used the formula

$$(a_0 b_2 - a_2 b_0)^2 + (a_1 b_0 - a_0 b_1)(a_1 b_2 - a_2 b_1) = 0$$

for the eliminant of

$$a_0 r^2 + a_1 r + a_2 = 0,$$

$$b_0 r^2 + b_1 r + b_2 = 0.$$

The computation is of great length, and I have not completed it. It results in an equation of the 6th degree in p and v , of which the terms of the 6th and 4th orders are as follows:—

$$\begin{aligned} & p^2 (\lambda v)^4 n^2 e^2 R + p^4 e^2 R^4 \\ & - p^2 (\lambda v)^2 \left\{ \frac{e^3 R^3}{\rho} (2\nu - 3n) + n e R^2 \left(1 + \frac{R}{\rho} \right) - 4 n e^3 R^2 \right\} \\ & + (\lambda v)^4 e^4 n \frac{R}{\rho} \left\{ n \left(1 + \frac{R}{\rho} \right) - \nu \left(1 + \frac{2R}{\rho} \right) \right\} \\ & + \dots\dots\dots = 0. \end{aligned}$$

These terms may be written shortly in the form

$$ap^2 v^4 + bp^4 - cp^2 v^2 + dv^4 + \dots\dots\dots = 0.$$

There can be no asymptotes except such as are parallel to the axes.

Equating to zero the coefficient of the highest power of each variable, we find the following asymptotes :—

$$\begin{aligned}av^2 - c &= 0, & v^2 &= 0, \\ap^2 + d &= 0.\end{aligned}$$

With respect to $av^2 - c = 0$, it would seem that we cannot have an infinite value of p corresponding to a finite value of v ; $\therefore c = 0$.

This gives the condition

$$\frac{2\nu}{n} = \frac{\rho}{R} \left\{ 1 + \left(1 - \frac{R^2}{e^2} \right) \left(1 + \frac{R}{\rho} \right) \right\};$$

$\frac{e}{R}$ is the current which would be produced through the armature, if not excited, by the given difference of potential. $\frac{R}{e}$ will generally be small. Neglecting its square, we have

$$\frac{\nu}{n} = \frac{\rho}{R} \left(1 + \frac{R}{2\rho} \right) = \frac{\frac{R}{2} + \rho}{R},$$

a value very close upon the boundary of stable arrangements, as we see from the investigations under the head of $E = \text{constant}$.

Then all the asymptotes parallel to $v = 0$ coincide with it.

With respect to $ap^2 + d = 0$, these asymptotes may have real values if they can; then d is necessarily negative.

$$\therefore \nu \left(1 + \frac{2R}{\rho} \right) > n \left(1 + \frac{R}{\rho} \right);$$

or, as $\frac{R}{\rho}$ is small, practically $\nu > n$, which is in conformity with practice and with the above condition.

Then for high velocities p would require to have the constant value $\sqrt{d/a}$.

The governing function of the type $p = \text{constant}$ is stable for most cases. It is intermediate in character between $p = kv$ and $p\nu = k$. It obviously requires that the pressure on the piston should vary inversely as the velocity.

In partially discussing these cases it has been more my object to throw light upon the general theory than to obtain practical results. The great value of theoretical investigation lies in the suggestion of new ideas; and if the above only serves to point out the importance of the construction of governors in accordance with definite laws, it will not be without use.

XLII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 223.]

February 21, 1883.—J. W. Hulke, Esq., F.R.S., President,
in the Chair.

THE following communications were read :—

1. "On the Relation of the so-called 'Northampton Sand' of North Oxfordshire to the Clypeus-Grit." By Edwin A. Walford, Esq., F.G.S.

The objects of the paper were said to be twofold :—in the first place to show the existence of some hitherto unrecognized beds of the Inferior Oolite in North Oxfordshire, and then to endeavour to define their position by comparison with one of the uppermost of the Cotteswold subdivisions, the Clypeus-grit. The area under discussion was said to be for the most part embraced in quarter sheet 45 N.W. of the Geological Survey, in the N.E. corner of which is situate the town of Banbury, whilst to the extreme S.W. lies Chipping Norton. The author first called attention to some remnants of a series of oolitic limestones at Coombe Hill, near Deddington, which he considered to be the equivalent of the Oolite Marl. He then pointed out near Bourton-on-the-Water the intervention of some sandy limestones and carbonaceous clays between the Clypeus-grit and the Fuller's Earth; he thought they might possibly represent beds found above the Clypeus-grit near Chipping Norton. The beds marked in the map 5' g 7', hitherto termed Northampton Sand, he said were well shown in the new railway-cutting at Hook Norton, and were capable of being split into several divisions :—the two thin base-beds containing *Ammonites læviusculus* and corals; the next higher (series C) yielding a large fauna, amongst which were *Rhynchonella spinosa*, *Trigonia signata*, and a doubtful fragment of *Ammonites Parkinsoni*. These, with a higher series of sandy, marly, and siliceous limestones, designated D and E, were proved to extend over the high lands to the S.W. It was shown that at one end of a ridge called Otley Hill the beds C rested on the Upper Lias, whilst on the S.W. flanks of the ridge the Clypeus-grit was to be seen also resting upon the Upper Lias. A road-section near Over Norton, he said, showed beds similar in lithological character to C and D of Hook Norton, resting upon the Clypeus-grit and evidencing a fauna of a somewhat similar character. The author thought that the almost unfossiliferous series E, which had been called the Chipping-Norton Limestone, might probably be found to be the equivalent in time of part of the Fuller's Earth, or of some of those beds of the Inferior Bathonian of the Côte d'Or described by M. Jules Martin.

2. "Results of Observations in 1882 on the Positions of Boulders relatively to the Underlying and Surrounding Ground, in North Wales and North-west Yorkshire; with Remarks on the Evidence they furnish of the Recency of the Close of the Glacial Period." By D. Mackintosh, Esq., F.G.S.

The author began by showing how boulders may be regarded as

natural time-measurers by their protecting the rock-surface underneath from the action of rain, which, around the boulders, denudes the surface, especially on the leeward and windward sides, where hollows resulting from pluvio-torrential action may generally be seen. He then described and explained the origin of the different forms of *supports* under boulders, which graduate from flat surfaces to *pedestals* of various forms, which he divided into *appropriated* (or preexisting), and those *acquired* through the boulders protecting the underlying rock from denudation. The author then described the positions of boulders on the high and uninhabited Eglwyseg limestone plateau near Llangollen, where it is certain they had never been disturbed by man. There he found that the average vertical extent of denudation by pluvial action around the boulders, since their arrival, was not more than six inches. After endeavouring to account for the fractured and crushed condition of the rocks under these boulders by precipitation from floating ice, he gave an account of his discoveries on the high limestone plateau north-east of Clapham (Yorkshire), where there is a "ghastly array" of many hundreds of large Silurian grit and slate boulders, nearly black in colour. From many facts and considerations the author endeavoured to show that most of the pedestals of these boulders must have existed *before* the arrival of the boulders, while the pedestals *acquired* through the boulders protecting the underlying rock from denudation, were generally imperfectly formed. On the Clapham plateau he found that the *average* vertical extent of denudation around the boulders with acquired pedestals was not more than on the Eglwyseg plateau, or about six inches. In the case of boulders which were not well adapted to concentrate rain-water, the extent of lowering of the surrounding rock-surface was often inappreciable; and this accounted for the continuous extension of flat limestone-rock surfaces under some of the boulders. The author then described what he had found to be *preglacial* as well as postglacial rain-grooves on limestone-rock surfaces, near Minera and on Halkin mountain (North Wales), where he found the average depth of those of the grooves which were probably postglacial to be about six inches. In conclusion the author entered into a consideration of the time which has elapsed since the close of the glacial period, and stated the main results of his observations as follows:—

1. That the average *vertical extent* of the denudation of limestone rocks around boulders has not been more than six inches.

2. That the average *rate* of the denudation has not been less than one inch in a thousand years.

3. That a period of not more than six thousand years has elapsed since the boulders were left in their present positions by land-ice, floating ice, or both.

3. "Notes on the Corals and Bryozoans of the Wenlock Shales (Mr. Maw's Washings)." By G. R. Vine, Esq.

XLIII. *Intelligence and Miscellaneous Articles.*

ON CENTRAL FORCES AND THE CONSERVATION OF ENERGY.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

MR. BROWNE asserts that my criticism of his paper in the January number of the *Philosophical Magazine* was not justified, on the ground that he had shown on page 38 that there could not, consistently with the conservation of energy, be a component at right angles to the straight line joining the two particles. Now the assertion that such a component, if it exists, will produce a rotation of B round A, which will increase B's kinetic energy, and that this increase will go on for ever, is only necessarily true if it can be shown that the component is not a function of θ ; for if it be a function of θ it will not necessarily tend to cause rotation continuously in the same direction. I did not allude to this in my communication to the February number, because in the argument which I criticised no mention was made of the fact that the non-existence of a component at right angles to the joining line must be assumed before it can be shown that the component in that line is a function of the distance only; and therefore I could not tell whether Mr. Browne considered this remark to justify his previous argument, or to be justified by it. However, even if it be proved that the component in the joining line is independent of θ , it does not appear to follow that the component at right angles, if it exist, is also independent of θ . I have the honour to remain, &c.,

G. W. VON TUNZELMANN.

ON THE REFRACTION-INDICES OF GASES AT HIGH PRESSURES.

BY J. CHAPPUIS AND CH. RIVIÈRE.

The constancy of the refractive power of gases, assumed by Biot and Arago after experiments in which the variation of the pressure did not exceed 1 atmosphere, has not hitherto been verified.

Since that period the problems stated by Arago respecting the refraction-indices of gases have been the subject of important investigations; but the physicists who have occupied themselves with these questions have regarded them chiefly as astronomical, and their aim has been to furnish the data which are indispensable for the calculation of atmospheric refraction; hence their experiments have been confined within the limits of atmospheric pressures.

The remarkable experiments of Andrews on the liquefaction of carbonic acid led him to the observation of some interesting phenomena produced in the gaseous mass at the moment of its change of state. The well-known appearances described by him cannot but be due to variations in the index of refraction of the gas. Moreover the disappearance of the meniscus at what has been called the *critical temperature* proves that at that instant the substance under experiment has only one index of refraction, while at lower temperatures there are to be distinguished the index of the liquid and the index of the gas.

These facts appeared to us to give some interest to a study of the indices of gases at high pressures.

After various trials, the interference method employed by M. Jamin, and described in his classical memoirs on the variation of the indices of air, water, and aqueous vapour, was selected by us as being the most precise; but it left us confronted by a difficulty which it had not had to surmount: the apparatus employed in previous experiments had only to support pressures, at the most, equal to the atmospheric pressure, while we wished to reach 60 or 70 atmospheres. We decided the question by using an apparatus already described and employed by one of us*, and which a modification in certain details sufficed to render suitable for the sort of experiments we wished to attempt.

This apparatus, constructed by M. Golaz, permits the compression of the gas under examination to take place in a prismatic cavity pierced in the interior of a block of steel of 20 centim. length and closed at each end by a glass plate of 1 centim. thickness, firmly fixed. One of the interfering pencils furnished by M. Jamin's first mirror traverses this cavity; the other pursues, in free air, a parallel path at a distance of 1 centim. Two glass plates identical with the first were interposed in the path of this second pencil. The two pencils afterwards pass through a compensator, and are received upon the second mirror, where they interfere. The fringes are observed horizontal and directed into a telescope furnished with a reticule.

In a first series of experiments we were able to follow the central fringe (white light) up to 65 atmospheres. Desiring to reserve to ourselves the time necessary for making these measurements with all the care which they require, we shall at present give only the result of measurements made between 24 and 36 atmospheres. In the third column of the annexed table will be found the number of the fringes (yellow λ of sodium) which pass under the reticule of the telescope for a variation of pressure given by the difference of the numbers in the first and second columns. Temperature 22°.

Pressure.		Number of fringes.	<i>n</i> .
Initial.	Final.		
24·5 atm.	28·5 atm.	335	0·550
28·5 „	32·5 „	311	0·510
32·5 „	36·5 „	338	0·555

We have calculated the number *n* of the fringes which would pass under the reticule for a variation of pressure of 1 millim. of mercury in a tube of 1 metre length; the results of the calculation are indicated in the fourth column.

The pressures were estimated with the aid of a metallic manometer which, unfortunately, had not sufficient precision to permit more regular results to be obtained. Nevertheless the numbers in the fourth column differ but little from the number 0·556, which is calculated on adopting for the index of air at 22° the value 0·000271, found by M. Jamin at the atmospheric pressure.—*Comptes Rendus de l'Académie des Sciences*, March 12, 1883, t. xvi. pp. 699–701.

* J. Chappuis, "Etude spectroscopique sur l'ozone," *Annales Scientifiques de l'Ecole Normale supérieure*, 2^e série, t. xi. April 1882.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

MAY 1883.

XLIV. *Optical Combinations of Crystalline Films.*

By LEWIS WRIGHT*.

[Plate VI.]

THE object of the following experiments is to illustrate the facility with which simple combinations of mica-films, such as can be readily put together by any one with the aid of Canada balsam dissolved in benzol, may be made to demonstrate not only the simpler chromatic phenomena of polarized light, but also the more beautiful and complicated appearances encountered at a more advanced stage of study. The colours obtainable from such mica-films are more delicate and intense than the usual selenite preparations, because while in selenite those films which produce the lower and more intense orders of Newton's colours are so thin as to be split with difficulty, in mica they can be obtained with the greatest facility. Some of the preparations are also, as demonstrations, superior in themselves.

Let us take first the simplest case, of different retardations produced by different thicknesses of crystal, counteracted or not by opposite retardations caused by another crossed film. It has been usual to demonstrate these by two selenite wedges, rotating one over the other. A simpler and more effective demonstration is given by two wedges, each built up of similar mica-films superposed, and cemented together, like those on the

* Communicated by the Physical Society.

screen (Pl. VI. fig. 1). The series of flat steps or tints are both more conspicuous and more readily understood; and if the two wedges are properly matched, when crossed the diagonal row of squares will be black when the Nicols are crossed. When the thick edge of one is superposed over the thin edge of the other, with the mica axes parallel, we have an even tint; and when the thick edge is superposed over the other thick edge, with the mica axes crossed, the retardations or colours produced by the first wedge are all destroyed by the second, and the field is all black. I wish to remark here, as I have done elsewhere, that the idea of wedges and other designs, built up in this way of thin flat films, is not due to me, but to my friend Mr. Fox, F.R.M.S., from whom both this pair of wedges and the next one are simply copied.

Here is another preparation of the same character, built up of twenty-four films, each of a thickness causing exactly $\frac{1}{8}\lambda$ of retardation for yellow light. Of course the thickness must be very exact to bear multiplication twenty-four times without sensible error. Now if we superpose on this wedge a flat plate of mica with its axis* crossing that of the wedge, and of a thickness equal to the middle stripe, that central stripe must appear black when the Nicols are crossed (fig. 2), while Newton's first order of colours and half the second order appear symmetrically on both sides of the black stripe. On one side of the stripe the wedge itself gives greater successive thicknesses; while on the other side the plate of mica does the same.

Such a wedge has further and real optical uses. It shows at a glance the precise composition of every successively increased $\frac{1}{8}\lambda$ of retardation for the first three orders of Newton's colours. When extinction is complete for yellow light, we know that a little of both red and blue must be unextinguished, the two giving us at the end of the first order the opaque plum-colour known as the "tint of passage." As the red at a given distance from the end of the spectrum is visually more conspicuous than the blue, at the end of the second and third orders this "tint of passage" must become more and more red, as we see on the screen is the case. The precise composition of the light destroyed and that remaining, we may demonstrate by placing a slit across the wedge and throwing its spectrum on the screen (fig. 3), when we see the shifting of the bands in steps for each $\frac{1}{8}\lambda$ of retardation. The wedge alone shows only the first three orders; but it is obvious that by superposing a plate of mica 1λ in thickness, the spectrum

* Throughout this paper the "axis" of the mica is supposed to be that one of its two polarizing planes which passes through the two optic axes.

would give us from the second to the fourth orders, and so on. I have not here the plates to show this in detail; but I have brought a thick plate of selenite, not measured, but hurriedly mounted for this afternoon. We throw its spectrum on the screen first: from the seven or eight dark bands it appears to be from eight to ten waves in thickness; it is at all events so thick as to show no colour. But now superposing the wedge, the shifting of the bands shows the precise composition for every successive $\frac{1}{8}\lambda$ of retardation even in this high order of interferences. It is all rendered by spectrum analysis. Another great use of such a wedge is for gauging the thickness of films in making other preparations, for which I use it constantly: we only have to superpose the film to be gauged with its axis crossing that of the wedge, and the stripe that is nearest extinction when the Nicol is crossed gives the thickness.

The *rotatory* colours of films are also beautifully shown by mica preparations. We all know that if a film $\frac{1}{4}\lambda$ thick (the terms "thick" or "thickness" of course mean in retardation of the slowest ray, throughout this paper) is adjusted with its polarizing planes at 45° with the plane of polarization, we obtain a single circular vibration. But if we adjust in this position a film giving colour next the polarizer, and introduce after that the $\frac{1}{4}\lambda$ plate, with its planes at an angle of 45° with those of the colour-film, both the two rays which emerge from the first film are converted into rays circularly polarized, but in opposite directions; and hence we get approximately the rotatory colours of quartz as the analyzer is rotated. The geometrical figure I now insert is thus circularly polarized, and will illustrate not only the beautiful rotational phenomena of the colours, but also that superior delicacy and intensity of these lower-order colours which has been alluded to: it would be exceedingly difficult to get colours like these in selenite. Again, we take the 24-section wedge used just now, and superpose upon it a $\frac{1}{4}\lambda$ plate made in two halves, one of which has its planes reversed as compared with the other; on rotating the analyzer the colours appear to pass along the wedge in opposite directions, as if it were made in two halves of right- and left-handed quartz.

My friend the Rev. P. R. Sleeman lately suggested to me another preparation, which was in turn suggested to him by a beautiful one in quartz belonging to the President of the Royal Society. This is a quarter-wave plate divided into twelve sectors. In the position now on the screen the polarizing planes are all perpendicular and horizontal; but the principal plane or "axis" is *reversed* (as in fig. 4) in every alternate sector. If we superpose this upon a mica-film giving

uniform colour, on rotating the analyzer we get, as you see, the contrary quartz rotations. But it lately occurred to me that a still more beautiful demonstration of these rotational colours would be obtained by another combination, which deserves perhaps to be called an "optical chromatrope." We place first in the stage next the polarizer a large even-tint film in a rotating frame; next to that a concave selenite plate showing Newton's rings; next to that again our quarter-wave plate in sectors. As we rotate the analyzer, one set of alternate sectors of the rings approach the centre, while the intermediate sectors recede from it; and if we now at the same time rotate the even-tint plate, we simultaneously vary the *colour* phenomena in an exquisitely beautiful manner.

A $\frac{1}{4}\lambda$ plate divided into four sectors or quadrants, with their planes alternately reversed in the same way (fig. 5), enables us to demonstrate the nature of the curious modifications of the rings and brushes in a plate of crystal when circularly-polarized convergent light is employed. Here, for example, are the rings and cross of calcite: interposing a $\frac{1}{4}\lambda$ plate, the black cross disappears into a grey nebulous one, and on opposite sides of each arm the quadrants of rings appear dislocated, the dark rings of one quadrant opposing the light rings of its neighbours. Interposing another $\frac{1}{4}\lambda$ plate on the other side, on rotating the analyzer one opposite pair of quadrants contracts while the intermediate ones expand, so that in two complementary positions we have unbroken circles. The same phenomena precisely are exhibited by this disk of chilled glass in parallel light, the gradually decreasing elasticity of the glass as we recede from the centre having the same effect as the increasing convergence of the rays has in the calcite. Now it is pretty easy to explain this phenomenon to a student by such a diagram as this (fig. 6) representing our crystal or glass with the Nicols crossed. The circularly-polarized ray we know is, on entering the glass, decomposed into its two plane-polarized components, of which one (let us suppose that denoted by the arrow-heads) is retarded a quarter of a wave. But the calcite or glass, beside this, itself also retards either the radial or the tangential vibration more than the other component—in calcite the radial. Taking, then, any originally-circular ring caused by the calcite retardations alone, we see that in two opposite quadrants the $\frac{1}{4}\lambda$ plate retards the radial vibrations a further quarter-wave, while in the alternate quadrants it accelerates them a quarter-wave. The result must obviously be a half-wave dislocation. As I have just observed, such a diagram sufficiently explains it all; but it seems to me better actually to represent it optically, by introducing the

composite quarter-wave plate, with its planes at 45° with the plane of polarization, before a film ground concave to show Newton's rings. Here we have, in an analogous way, in opposite quadrants retarded one of the component vibrations a quarter-wave before entering the selenite, while in the alternate quadrants we retard the other component; and we get similar dislocations. Again, letting the concave selenite come first, and superposing a $\frac{1}{4}\lambda$ plate cut in quadrants with their planes alternately horizontal and vertical, we now have the contracting and expanding quadrants, with the perfect circles in two positions, as in the calcite. We may make the demonstration complete by reversing the process, and superposing our last composite $\frac{1}{4}\lambda$ plate on the disk of chilled glass*. We now are applying in each quadrant all the retardations equally to either the tangential or radial vibrations; and hence the rings remain perfectly concentric, while they expand or contract as the analyzer is rotated: there is no dislocation at all. Finally, either the quadrant or 12-sector $\frac{1}{4}\lambda$ plate superposed on this *square* of chilled glass gives us a very beautiful demonstration that the dislocation of the crystal rings is entirely due to the $\frac{1}{4}\lambda$ plate retarding one component ray in the crystal on one side of the plane of polarization or that at right angles to it, and accelerating the same component on the other side of those planes. Here we have the square perpendicularly adjusted, with the composite plate superposed. When the analyzer is rotated, the reversal of the sectors on the lines of the black cross keeps the figure symmetrical, as in the last experiment. But you observe that the *diagonals* of the square are covered, each by a single plate or sector; and a mere glance at the screen makes it obvious that if, *in this position*, these diagonals were covered—as the black cross now is, by the junction-line between two contrary sectors, they would be dislocated, the colours on one side of the line approaching the centre, and those on the other receding when we rotate the analyzer. But we will now bring these diagonals of the square into the planes of polarizer and analyzer crossed, and superpose the sectors again upon the glass, junction-lines now covering the diagonals. You observe that the state of things is exactly reversed; and the contrary sectors now do keep the figure symmetrical on each side of the diagonals, while, on the other hand, the single $\frac{1}{4}\lambda$ plates which now cover the bisecting diameters of the square preserve the

* In private experiment we can of course do this with a plate of calcite; but in a projecting instrument it is rather difficult to ensure the precise axial coincidence of all the arrangements with the axis of the convergent light, without which the experiment fails.

symmetry there also. It is not necessary to add details of explanation which will be familiar to all.

Allow me next to illustrate the beautiful phenomena of crossed films of mica in highly convergent light, such as will take in biaxial angles of, say, 50° . Our starting-point will be Norremberg's beautiful discovery, worked out entirely from theory, that by crossing films of biaxial mica of gradually increasing number and proportionately diminished thickness, there was a gradual approach to the rings and cross of a uniaxial crystal. He found three wave-lengths of retardation the best approximate unit. Here is a single plate of mica—the ordinary biaxial lemniscates; and here are two such plates crossed at right angles—the ordinary figure of a “crossed” crystal, in which we get the black cross. With four plates crossed we get the first approach towards rings, each of the “eyes” being now bisected by a straight fringe placed as a tangent to the figure. Norremberg's next preparation was eight films crossed; but I add one of six, which gives a single perfect though nearly square ring, while eight films give two rings. Twelve give three rings and signs of a fourth; while twenty-four, as you see, are absolutely undistinguishable from a calcite. The whole series will be thus:—

$$\frac{1}{3\lambda}, \quad \frac{2}{3\lambda}, \quad \frac{4}{4\lambda}, \quad \frac{6}{\frac{1}{2}\lambda}, \quad \frac{8}{\frac{3}{8}\lambda}, \quad \frac{12}{\frac{1}{4}\lambda}, \quad \frac{24}{\frac{1}{8}\lambda}.$$

Now there is no necessity for an *exact* total thickness of three wave-lengths in constructing this series; but an approximation to it is necessary, to preserve the gradation of the phenomena and the gradual passage to the uniaxial figure. So far Norremberg ascertained; but he does not seem to have carried his experiments with mica any further. Let us now do so. The eight films gave us two rings, the outer one squarish in figure. But if we combine eight very *thin* films (say $\frac{1}{8}\lambda$ thick, as in this preparation), you observe that we get perfectly circular rings at once; and in fact even four very thin films will give them; and twelve thin films give us quite fine circles. Now, on the other hand, let us employ four and eight *thick* films—in this case over 1λ thick (we thus more than double Norremberg's thicknesses); and observe that the rings now have altogether disappeared, and the curved fringes are all turned the reverse way, their convex sides to the centre. The same thing is still more evident in this splendid figure, produced by twelve crossed films $\frac{3}{4}\lambda$ in thickness. We see easily enough that it must be so, if we follow in our minds the decompositions and recompositions of the vibrations in traversing the successive films; but it is very interesting to notice how,

with the same number of micas crossed in exactly the same way, but of different thicknesses, the phenomena appear actually reversed in character. Having seen this, we abandon simply crossed films, and the following will be composed of films superposed at angles of both 90° and 45° . Here another cause of variety comes into play, since all films whose thickness contains an odd $\frac{1}{4}\lambda$, when superposed at 45° will circularly polarize the light. Moreover we also know that if two such films are superposed at an angle of 45° , the effect is to rotate the plane of polarization itself (as shown for instance by the rotation of a calcite cross) 45° from the original plane. Hence the variety and scope for combination here are endless, the phenomena always being beautiful; but I must only show you a very few of such preparations. The first four are all composed of films $\frac{1}{2}\lambda$ in thickness, and each contains the same number of twelve films, and the lines show the successive positions of the mica "axis." In the first they are

| \ | — \ — | \ | — \ —

Thus all the diagonal axes lie the same way. Now this second preparation has the very same individual films differently placed, thus:—

| \ — | \ — | / — | / —

You see the total difference in effect produced by the difference in crossing.

The next one is thus arranged:—

|| — || — | = | =

This is an interesting combination, because the wave-decompositions indicate that the light should be nearly extinguished when the Nicols are crossed, not only in the original black cross, but also along the diagonals between. You see that it is so; but this result is still more completely brought out by the next preparation,

| — | — | — — | — | — |

where we get a nearly perfectly black square crossed by nearly black diagonals as well as by the black cross. The next set of five are all built of films one wave in thickness, as follows:—

No. 1.	8 micas	— × —
2.	10 „	+ + × + +
3.	12 „	+ + × × + +
4.	8 „	+ × + ×
5.	8 „	/ — \ / — \ (i. e. successively rotated 45°).

This last figure is interesting, because we can see that the result must be some polygonal or roughly circular central figure with some sort of a cross, surrounded by eight detached figures or eyes. I am sorry I cannot work this out mathematically; but with whole-wave and somewhat greater retardations it is pretty easy to trace it in one's own mind. It is so, as you perceive; and you also see that the preparation and figure can be rotated without very sensible change, which also follows from theory, and is a somewhat remarkable result, after what we saw at the commencement, with such thick films. And now, to show the effect of thickness, here is a precisely similar preparation of eight films superposed at a successively rotated angle of 45° , but built up of $\frac{3}{4}\lambda$ films. Circular polarization here comes into play; and the effect is totally different in every way. The last of these crossed micas is built up of twelve $\frac{3}{4}\lambda$ films, thus—

$$\times + + + + \times$$

You see the total difference in figure from any thing before, and the scope for endless variety, which I must not further pursue.

Still more beautiful, but perhaps less interesting, are the combinations of mica- and selenite-films discovered experimentally by Norremberg. As he observes, if we call the three axes of elasticity in any crystal x, y, z , then selenite-films contain x and z , while mica on the other hand contains y and z ; and it is easy to see that if preparations are built up of both elements, very fine coloured fringes must result, differing very greatly in character according to whether the x of the selenite is parallel to the z of the mica or crosses it. As far as I remember, however, Norremberg and Reusch seem to have said that the characters of the fringes defy all prediction. This is perhaps hardly true, even apart from mathematical analysis, which I am unable to give, and which the mere beauty of these combinations is scarcely worth. For it is easy to perceive that if a single selenite be placed between two thick micas, we must have very nearly the usual biaxial figure, with some little modification in the eyes or rings, but chiefly distinguished from the simple mica by rich colour. That is so here. But if we alternate several parallel selenites between parallel micas of less thickness, so as to give the selenite functions of elasticity more comparative influence, then it is evident that the modified lemniscate curves, or what is still traceable of them, must be either brought nearer together or more widely separated, and that we shall thus obtain curved fringes having their approximate origin in the original

optic axes of the mica, but reversed in character according as the x of the selenite crosses or is parallel to the mica z . This is the simplest analysis I can give, and it follows on consideration from Norremberg's data. Here are two such preparations, in each of which four selenites are alternated between five $\frac{1}{2}\lambda$ micas. In the first the modified lemniscates are wider apart than in the mica, but the resulting fringes originate approximately in the mica axes. In the second the selenites are at right angles with their former position, and the fringes still centre in the axes, but the curves are reversed; and the resulting "palm-tree" pattern is perhaps one of the most beautiful, both in colour and figure, which it is possible to behold.

The few other preparations here are built up of either four or six ternary elements constructed on Norremberg's system, each consisting of two parallel micas, with a selenite between them either crossed or parallel. In the first you readily recognize the "palm-tree" character of the last figure, "crossed." It is needless to describe the others; for here, too, variety is boundless; but I purposely reserve for the last, two combinations composed of exactly the same arrangements of both mica and selenite, and all the micas the same ($\frac{1}{2}\lambda$) thickness, but the selenite films in one slightly thicker than in the other. The difference in effect is purposely not so great as to prevent your recognizing the same general figure in both, but is still conspicuous and interesting. Let me, in conclusion, hope that the beauty of these preparations constructed after Norremberg's method, and the facility with which they can be prepared, may make them better known.

[At the conclusion of the paper Mr. Wright described and exhibited an adaptation to the microscope by Messrs. Swift and Son, by the aid of which all the preparations and crystals requiring highly convergent light could be shown on the stage of any microscope constructed with a draw-tube.]

XLV. *On Permanent Magnetism.*

By R. H. M. BOSANQUET.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN a former paper I have proposed to follow out Faraday's analogy between magnetism and electricity; and the following is a summary of the resulting point of view.

Magnetic induction, or the number of lines of force through unit area, is the quotient of magnetomotive force by resistance, in magnetic circuits.

Magnetomotive force is a difference of magnetic potential. For any circuit once linked with n spires of an electric current, the magnetomotive force is $4\pi Cn$.

Magnetic resistance is a linear quantity; for the dimensions of magnetomotive force are

$$[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1}];$$

and of magnetic induction,

$$[L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1}].$$

The idea of *magnetizing force* as the cause of magnetic induction is abandoned. Magnetizing force is regarded simply as magnetic induction in space or air. Magnetic induction is conceived of as arising directly from the action of magnetomotive force on circuits of magnetic resistance.

It was shown in my previous paper, that if a permanent magnet of hard steel be divided into short lengths, the sum of the moments is not the same as before, but is greatly reduced. This contradicts the fundamental statement generally received. The chief object of the present paper is the further discussion of the experiment in question.

It was suggested that, according to the Ampèrian analogy, permanent magnets may be expected to have permanent magnetomotive forces in their substance; and these acting on the magnetic resistance of the circuit would produce magnetic induction, depending partly on the resistance of the steel, and partly on that of the return through space or air. These considerations not only explain the phenomenon in question, but enabled me to foresee it.

I will shortly recapitulate the experiment. A compound magnet is constructed of eighteen cylindrical pieces, fitted, hardened, and magnetized.

	centim.
Whole length . . .	= 28·50
Length of each piece .	= 1·58
Diameter	= 1·97 = 2R'.

These are placed in a cradle hung in a bifilar suspension arranged so that the plane of zero deflection is at right angles to the magnetic meridian. Then, for weeks, there were obtained in the mean:—

Deflection due to 18 pieces joined up .	= 13°·1
" " " separated .	= 1°·8

In the first instance the mean deflection of 1°·05 was obtained with the separated pieces, as stated in my first paper. I have come to the conclusion that this was due to some error in handling the apparatus. When it is at all disturbed the tor-

sion of the wires changes rapidly, and affects the results. The condition of the instrument can always be ascertained by making observations in pairs, so as to give zeros. If these agree, confidence may be felt. With bad wires I have had two consecutive zeros differ by several degrees.

As to the use of the bifilar for absolute measure, I will only observe, with reference to a recent paper of Kohlrausch*, that I share his predilection for the method, though I do not attempt to attain his degree of accuracy. But one part of his work seems inadmissible for accurate purposes; and that is the determination of the torsion by calculation, or by experiment with a separate piece of the suspension wire. In my experience wires differ. I determine the torsion of the wires in use by balancing it against some approximately known magnetic moment, the torsion just exceeding 180° . It must be determined both ways (right and left). It is impossible to assume that the zero of torsion coincides with the zero position. If it did so once, it would not continue to do so after a weight had remained deflected in the bifilar for a day or two, unless with an exceptionally good wire. There is continual change in consequence of what the Germans call "*elastische Nachwirkung*," which I may describe as "imperfect elasticity." I suspect that a change of this kind had something to do with the change noticed in the note at p. 215 of my previous paper.

I may just mention that the magnetic meridian is most conveniently determined by hanging an electromagnet in the bifilar, by the method subsequently described. When on reversing the current no change of position takes place, the axis is truly in the magnetic meridian. The moment of the electromagnet must be a little less than that of the bifilar.

The difficulty in making any deduction from the observed ratio of the moments of the compound permanent magnet, when joined up and separated, consisted in our ignorance of the amount of the air-resistances of the magnetic circuits. I have succeeded in completely determining these by means of soft-iron electromagnets, having nearly the same dimensions as the steel, both joined up and separated. The pieces of soft iron only differ slightly in diameter from the steel. The difference was an oversight. The diameter of the soft iron is 1.90 centim., that of the steel 1.97. Each length of 1.58 centim., whether separate or forming part of a long bar, is wound with ten turns of wire.

A mercury contact is arranged under the cradle, by which currents can be transmitted to the coils with little friction. It was found that semicircular channels, with dipping wires,

* Wiedemann's *Annalen*, xvii. p. 737.

caused so much friction that the cradle would rest in any position within two or three degrees. Now there is a central cup or thimble with a central wire, and a circular channel round it, of small diameter, in which an excentric wire dips. The friction is not objectionable.

The course of experiment is this:—The bifilar is set at right angles to the magnetic meridian. A current is sent through, and the deflection measured. The current is measured by a new standard galvanometer, just constructed. It has ten turns, of which any number can be used, parallel or in series.

The current is reversed, and the mean of the deflections, corrected for torsion, taken. The moment is then calculated from the constants of the bifilar. The magnetic induction \mathfrak{B} is then calculated, employing the observed distance between the poles. The magnetomotive force, $(4\pi Cn)$, is calculated.

The resistance x is $\frac{\text{magnetomotive force}}{\text{magnetic induction}}$. The permeability of the metal for the given value of \mathfrak{B} is then taken from Rowland's table for soft iron (previous paper, p. 209). The length of bar divided by this permeability is the resistance of the metal, and is subtracted from x .

The remainder is the resistance of the air-circuit for the shape in question. Assuming that this resistance can be expressed in the form $2\alpha R$, we divide this remainder by R , and obtain 2α , the air-resistance for unit radius.

$2\alpha R'$, where R' is the radius of the steel, gives us the air-resistance for the shape of the steel. This process is carried out both for the whole bar and the separate pieces. These air-resistances are then introduced into an equation which states that the total resistances, joined up and separate, must be inversely as the observed moments. Hence the resistance of hard steel is found to be less than that of air in the proportion of about 1 : 31.

The resistance was then determined directly, by sending a current through a coil wound about the steel itself. As large a current as was safe was used. It increased the permanent magnetism a little at first, but afterwards there was no further permanent change. The resistance thus determined by temporary change of moment was to that of air nearly as 1 : 32. A former determination was rejected on account of a disturbance in the permanent magnetism. There is no further room for doubt that the behaviour of permanent magnetism in hard steel is completely explained by the assumption of magnetomotive force and resistance in the steel; and if the resistance be determined by division of the bar, the same numerical result is obtained as by direct determination.

The variation in the resistance of the steel under different values of the magnetic induction has not yet been regarded.

The change in the air-resistance for unit radius, from about $\cdot 45$ in the case of the thick disks to $\cdot 30$ in the case of the long bar, shows that the length of the bar has some influence on this quantity. It is proposed to examine further the law of change.

The temperature during the work was always high and tolerably constant. So far its variation has not been taken into account. In the early morning, before the room is warm, an increased moment is observed. The permanent magnet as it now stands in the bifilar has a deflection of about 14° , reading during the day $73^\circ \cdot 7$ (zero 88° nearly). I have gone in early on several mornings and found the reading to be about $72^\circ \cdot 4$. It returns to its former position as soon as the room is warm.

Heat therefore diminishes the magnetism*. It remains to be determined whether the magnetomotive force or the resistance is affected. In the latter case we should have a curious analogy to the behaviour of electrical conductors, the resistance increasing with the temperature.

So long as the permanent magnet was kept free from magnetic influences, its mean condition did not sensibly change. I wished to make it take a higher moment, and tried magnetizing it on the dynamo. It took a higher moment, but soon lost it again. I had reason subsequently to think that one of the pieces was reversed in this process, which threw doubt on the first direct determination of permeability. I then remagnetized it carefully on the dynamo. The moment soon fell again to about its original value; it was slightly increased by the first action of the currents used in the second determination of permeability, but did not change further during the determination, and still remains the same.

Calculation of Experiments.

Constants.

$$\left. \begin{array}{l} a = \frac{1}{2} \text{ inch,} \\ b = \frac{1}{4} \text{ inch,} \end{array} \right\} \text{ distance of points of suspension.}$$

$$h \text{ had the values } \left(\begin{array}{c} 41 \\ 37 \\ 34 \end{array} \right) \text{ inches, height of suspension.}$$

$$\left. \begin{array}{l} m = 41 \text{ oz.} = 1162 \cdot 3 \text{ grm.,} \\ g = 981, \end{array} \right\} \text{ weight.}$$

* This conclusion is opposed to that of Meyer (Wiedemann's *Annalen*, xviii. p. 246). His conclusion is founded on the difference between experiments in September and January. I measure the difference directly, and see the change repeat itself daily.

$H = \cdot 181$, horizontal component.

$f = 26$ centim., distance of poles of bar.

Length of bar = 28·50 centim.

Separate piece = 1·58.

$2R' = 1\cdot97$ steel.

$2R = 1\cdot90$ soft iron.

Fundamental Equations.

$$\text{Moment} = \frac{ab mg}{4hH} \tan \theta.$$

$$\frac{\text{Moment}}{f} = \text{Pole}.$$

$$4\pi \text{ Pole} = \pi R^2 \mathfrak{B}, \text{ or } 4 \text{ Pole} = R^2 \mathfrak{B};$$

$$\therefore \frac{4 \text{ moment}}{f R^2} = \mathfrak{B}, \text{ magnetic induction.}$$

$$\text{M.M.F.} = 4\pi Cn, \text{ magnetomotive force.}$$

$$x = \frac{\text{M.M.F.}}{\mathfrak{B}}, \text{ total resistance.}$$

$$x_b = \frac{\text{length}}{\mu}, \text{ resistance of bar } (\mu \text{ found from } \mathfrak{B} \text{ in Rowland's table}).$$

$$x_a = x - x_b, \text{ air resistance.}$$

$$2\alpha = \frac{x_a}{R}, \text{ air resistance for unit radius.}$$

$$2\alpha R', \text{ air resistance for radius } R'.$$

Soft iron bar.

$h = 37$ inches.

	I.	II.
Current	1·283	·83 ampère.
Deflection	21°·8	14°·0
Moment	5405	3369
\mathfrak{B}	921	574
x	·31495	·32685
μ	1100	842
2α	·30426	·30855

Mean value of $2\alpha = \cdot 30640$,

whence

$$2\alpha R' = \cdot 30181.$$

18 pieces of soft iron.

$h = 37$ inches.

	I.	II.	III.
Current	1·0	4·2	5·36 ampères.
Deflection ...	0°·7	3°·0	3°·75
Moment	165·2	708	885·77
\mathfrak{B}	28·1	120·74	151
x	·44645	·43714	·44607 for one piece.
μ	410	460	480
2α	·46590	·45654	·46610

Mean value of 2α = ·46285

Corresponding value of $2\alpha R'$... = ·45592

From II. alone $2\alpha R'$ = ·44970

I. has little weight, because of the smallness of the observed deflection; III. because the current was too much for the coils and the apparatus was heated. The value derived from II. alone is therefore to be preferred. I adopt $13^{\circ}\cdot 1$ and $1^{\circ}\cdot 8$ for the deflections of the steel compound bar, joined up and separate. In deducing the moments from these, h must be taken = 41 inches. The condition was very permanent; but the moments were lower than they are now.

Then we have, if r be the resistance of one piece,

$$\frac{18r + \cdot 302}{18(r + \cdot 456)} = \cdot 135.$$

Taking first the mean of I., II., III. for the soft iron; then

$$1\cdot 58 = \frac{1}{30\cdot 52}, \quad \mu = 30\cdot 52.$$

Taking II. alone for the soft iron,

$$\frac{18r + \cdot 302}{18(r + \cdot 450)} = \cdot 135,$$

whence

$$\mu = 31\cdot 06.$$

Direct determination of permeability of the permanent magnet.

$h = 34$ inches.

	Current 1·33 ampère.	Reversed.
Deflections	9°·9	21°·33
Moments	2566·7	5743·7
Diff. of moments	3177·0	

$$\mathfrak{B} \dots\dots 503\cdot7,$$

due to diff. of moments;

$$\begin{array}{r} x \dots\dots 1\cdot1944 \\ x_a \dots\dots \cdot3018 \\ \hline \cdot8926 \end{array}$$

$$\frac{28\cdot5}{\cdot8926} = 31\cdot93,$$

which is the value of μ thus obtained.

XLVI. *On a Method of Measuring Electrical Resistances with a Constant Current.* By SHELFORD BIDWELL, M.A., LL.B.*

IT sometimes happens that the resistance of a body appears to depend upon the strength of the current which traverses it. Thus the resistance of the carbon filament of an incandescent lamp may be several ohms lower when tested with a strong current than it is with a weak one. In this case there is little doubt that the difference is due only indirectly to the current itself, and is in fact caused by the heat which the stronger current develops, and which, even when the circuit is closed only for a moment, may produce considerable effect upon the conductivity of the filament. Again Prof. Adams, at an early stage of his well-known experiments with selenium, found that, on increasing the strength of the current through the selenium, there was a diminution in its resistance †. The same is the case with the mixtures of sulphur and carbon which I described in a previous communication ‡, and to a very much greater degree with loose contacts of carbon or metal, such as are used in the microphone. For example, a carbon pencil being arranged so as to rest at right angles upon another with a pressure of $\cdot05$ grm., the resistance at the point of contact was found to be 11·02 ohms with a current of $\cdot1$ ampère, and 68 ohms with $\cdot001$ ampère; and when cylinders of bismuth were substituted for the carbon, the resistances with the same currents as before were 5 ohms and 182 ohms respectively.

Without assuming that the resistances in these and similar cases are altogether true resistances, it is nevertheless sometimes convenient to treat them as such; and for purposes of

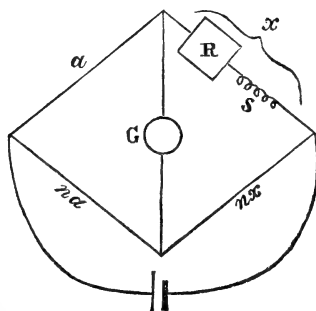
* Communicated by the Physical Society of London, having been read at the Meeting of the Society on March 10th, 1883.

† Phil. Trans. vol. clxvii. pp. 319, 342.

‡ Proc. Phys. Soc. vol. v. p. 90.

comparison it is clearly necessary that currents of known or constant strength should be used in their measurement. When the Wheatstone's bridge is employed in the usual manner, the current passing through the unknown resistance will, of course, vary with the magnitude of this resistance, being smaller when it is high than when it is low ; but by a very simple modification of the common arrangement, which I have used extensively during the last year, it is easy to ensure having currents of uniform strength throughout a series of measurements.

In the figure, x , nx , a , na are the four arms of a Wheatstone's bridge, S is the unknown resistance, and R is a box of



resistance coils which is inserted in the same arm. If E denote the electromotive force of the battery, B its internal resistance, and C the current which passes through the arm containing S , then, when there is a balance,

$$C = \frac{n}{n+1} \times \frac{E}{B + \frac{n(a+x)^2}{a+x+n(a+x)}}$$

$$= \frac{nE}{(n+1)B + n(a+x)}.$$

From this expression we can find what value x must have in order that the current through the unknown resistance may be of any definite strength. Having determined this value, we insert resistance equal to n times its amount in the arm nx , and adjust the resistance in the box R until a balance is obtained. We then know that the resistance of $R + S$ is equal to x ; that the resistance required to be measured, S , is equal to that of the arm x less the resistance employed in R ; and that a current of the desired strength, C , is passing through it.

A second unknown resistance may now be substituted and measured as before, simply by altering the resistance of R , with the certainty that when there is a balance the current is of the same strength as in the former case. The resistance nx remains unchanged throughout. It is of course necessary so to choose the values of a , n , and E that x may be greater than the resistance to be measured; and it is generally desirable that the resistance of the whole bridge should be made as high as conveniently possible.

The great advantage of this method over others that suggest themselves lies in the fact that, since it is never necessary to close the circuit for more than a moment, the electromotive force and resistance of the battery remain sensibly constant during a long course of experiments.

XLVII. *The Auroral Beam of November 17, 1882.*

*By J. RAND CAPRON, F.R.A.S., F.M.S.**

[Plate VII.]

NOVEMBER last (1882) was remarkable for the great auroral storm, or, rather, series of storms, which prevailed over a considerable portion of the globe during the latter half of that month.

Accounts of brilliant displays from the Shetland Islands, at Edinburgh, a great number of localities in England, Rome, and Florence soon made their appearance in the public prints; and to these later on were added others from America, California, Spain, Sweden, Belgium, the Netherlands, and other countries.

Continuous auroræ were seen at Trondhjem from the 12th to the 18th of the month; and I traced almost incessant displays in these latitudes from the 13th to the 24th at least—the motions of the telegraph-needles acquainting us that auroræ were not only present when actually visible, but by day and during clouded nights.

Also accompanying these were considerable groups of solar spots, one the largest ever seen at Greenwich, and a widespread disturbance by earth-currents of State telegraphic communication in nearly all the countries above mentioned. It was during one of these storms that, in the south of England and the adjacent parts of France and Belgium, a phenomenon was seen which, though apparently not without precedent in the annals of auroræ, was at least of a rare and striking character.

* Communicated by the Author.

About 6 P.M., while the aurora was fitfully blazing in the north, north-east, and north-western sky, in the east there rose from the horizon a long beam of detached bright light, which, apparently lengthening as it advanced, crossed rapidly the southern horizon in front of or near the moon, and then sank in the west, shortening in length as it did so. The light emitted from it was described by one observer as of a glowing pearly white; and the general effect of this huge shining mass sailing majestically across the sky, even upon those accustomed to kindred phenomena, was at least one of wonder and surprise, while in the less experienced in such matters it created a feeling of absolute awe. Indeed to such an extent in some instances did this latter emotion prevail, that two labourers in my neighbourhood, who separately witnessed it, thought "that surely the world was coming to an end."

In this general description of the "beam" of the 17th of November, I may add I have advisedly preferred to use the term "beam" (the "trabs" of Musschenbroëck, an oblong track parallel to the horizon) as a definition of the object, because in speaking of auroral rays and beams it is generally understood that the former are the spreading fixed shafts (*sagittæ*) which shoot from the arches or horizon towards the zenith, while the latter term is applied to the bright clouds passing at right angles to the former. Good examples of these were observed at Guildford on February 4, 1874, and are figured in 'Auroræ,' plate 6. Having regard to the almost unique nature of the phenomenon, it seemed to me a desirable thing to gather together the published accounts of its appearance, and to endeavour to trace something of its history more in detail and more precisely than they singly afford.

In a few of the public prints (notably in 'Nature') a number of interesting (and doubtless accurate, so far as the suddenness of the apparition would admit of) observations are recorded; but, on the other hand, it is somewhat strange that the scientific journals in general contain but little, if any, notice of it; and as to newspapers, while the 'Standard' and some few others contain scattered accounts, the 'Times' (probably from a want of due appreciation of the importance of the subject) published two letters only out of a "great number" it stated it had received. Regretting this, and using to the best of my ability the materials I have found at my command, I have drawn up a schedule of twenty-six observations at home and abroad, showing at one glance the prominent features in each observation and giving the authorities, that those so disposed may consult them for further or more exact details. They are as follows:—

Particulars of Passage of Auroral "beam" of 17th November, 1882. (Moon's position at Guildown, Guildford, at 6.14 P.M., R.A. $21^h 12^m 0^s$, Dec. S. $10^\circ 35' 0''$.)

No.	Reference.	Observer's name.	Station.	Time.	Latitude.	Longitude.	Time of flight.	Length.	Breadth.	Direction of flight.	Observer's notes.
Observations where stated to pass <i>above</i> the Moon.											
1.	'Standard' paper.	Railway signalman.	Sidmouth ...	h m 6 8	$50^\circ 42' 0''$ N. $3^\circ 15' 0''$ W.	Overhead, accompanied by much telegraphic disturbance.
2.	* Nature, vol. xxvii. pp. 84, 149.	J. R. Capron.	Guildown, Guildford.	Soon after 6.	$51^\circ 13' 39''$ N. $0^\circ 28' 47''$ W.	Little more than 1 minute.	E. to W. nearly.	About 2° above Moon, centre to centre; altitude of Moon above horizon $28\frac{1}{2}^\circ$; spindle-shaped: spectroscopic observation.
3.	83.	Astronomer Royal.	Greenwich...	6 4	$51^\circ 28' 0''$ N. $0^\circ 0' 0''$ W.	About 2 minutes.	E.N.E. to W.	Passed nearly along a parallel of declination. A little <i>above</i> the Moon; Mr. Maunder adds, "quite close above the Moon."
* To save repetition, in all cases where page numbers only are given vol. xxvii. of 'Nature' (in which the phenomenon is reported) is understood.											
Observations where stated to pass <i>across</i> the Moon.											
4.	Idem, pp. 100, 141, 412.	A. Batson ...	Ramsbury (Hungerford)	6 2	$51^\circ 25' 0''$ N. $1^\circ 30' 0''$ W.	Torpedo-like. Passed exactly across Moon's disk. Moon's position at Ramsbury:— R.A. $21^h 12^m 56^s$, N.P.D. $100^\circ 35' 7''$.

5. Idem, p. 87.	John J. Dobson.	Old Windsor.	6 5	51 29	0 N.	0 38	0 W.	50 sec.	30°	4°	E. to W.	Ship torpedo, or weaver's shuttle. Rose a little south of Aldebaran and slid along at same N.P.D., across face of Moon and disappeared in W. under Alair. Across face of the Moon. Moon's diameter above and below.
6. Idem, p. 141.	E. Pollock	Lincoln's-Inn Fields.	6 0	51 31	0 N.	0 6	0 W.	Less than $\frac{1}{2}$ min.	E. to W.	Passed across the Moon.
7. 'Standard.'	J. Woodruff.	Broxbourne (Hertford).	?	51 39	0 N.	0 1	0 W.	Exactly across the Moon; rose vertically below the Pleiades, like a cigar-ship.
8. Nature, p. 87.	Hubert Airy.	Woodbridge.	6 7	52 6	0 N.	1 20	0 E.	About 1 min.	40°	5°	E. to W.	Snowy disk of cloud sailing edge-wise; dark nucleus $3^{\circ} \times 2^{\circ}$; over (across) Moon's disk (moon shining through it).
9. Letter to Roy. Astr. Soc.	Wm. Munro.	Chatham	6 5	51 23	0 N.	0 35	0 E.	1 ^m 30 ^s	30°	3°	N.E. to S.W.	

Observations where stated to pass below the Moon.

10. Nature, p. 84.	Joseph Clark.	Street, Somerset.	Soon after 6.	51 7	0 N.	2 43	0 W.	Less than 4 min.	S.E. to S.W.	South of Moon, bar of yellowish light, "dark something" before the bar and dark streak where it passed. Same position as arch at Leeds. (Observation 22.)
11. Idem, pp. 86, 100, 338.	S. H. Saxby.	E. Clevedon, Somerset.	6 15	51 26	0 N.	2 52	0 W.	80 sec.	35°	3°	E. to W.	Rather below Moon. Altitude on meridian about 22° ; pale yellow-white. Trajectory much flatter than the stars.
12. Idem, p. 85.	T. G. Elger...	Hempston, Bedford.	6 5	52 7	0 N.	0 27	0 W.	About 6 sec.	20°	5°	S.E. to S.W.	Lenticular; axis parallel to horizon, passed below Moon. Spectroscopic observation of rosy streamers.

N.B. Where seconds are not given the latitudes and longitudes are only approximate.

Table (continued).

No.	Reference.	Observer's name.	Station.	Time.	Latitude.	Longitude.	Time of flight.	Length.	Breadth.	Direction of flight.	Observer's notes.
Observations where stated to pass below the Moon (continued).											
13.	'Standard.'	Thos. Woodruff.	St. Ives, Huntingdon.	?	52° 22' 0" N.	0° 17' 0" W.	5° or 6° below Moon.
14.	Nature, pp. 87, 146, 365, 434.	H. D. Taylor.	Heworth, York.	6 4	53° 50' 0" N.	1° 56' 0" W.	$\frac{3}{4}$ min.	30°	S.E. to S.W.	About 30° altitude on magnetic meridian. Under the Moon about 6° (?) centre to centre.
15.	Idem, p. 85.	A. M. Worthington.	Bristol	6 3	51° 26' 0" N.	2° 35' 0" W.	Hardly 1 min.	27°	1°	E. to W. to S.W.	Under Moon; rather less than three times Moon's diameter.
16.	Idem, p. 99.	H. McLeod.	Cooper's Hill.	?	51° 25' 57" N.	0° 34' 5" W.	Just below Moon. One observer thinks upper edge grazed lower edge of Moon.
Observations where an estimated altitude independent of position with regard to Moon is given.											
17.	Idem, p. 86.	E. Brown	Cirencester...	Just after 6.	51° 43' 0" N.	1° 58' 0" W.	Altitude about 30°. Shaft of intense white light.
18.	Idem, p. 87.	A. S. P.	Cambridge...	6 5	52° 13' 0" N.	0° 7' 0" E.	Less than 1 min.	Due E. to S.S.W.	Spindle-shaped. Subtended visual angle about 20°.
19.	'Times' paper.	C. M. Ramus.	Rye, Sussex.	About 6	50° 57' 0" N.	0° 43' 0" W.	2 min.	90°	E. to W.	About 50° above horizon; described as a cometary body.
Observations where no altitude is given.											
20.	Idem	J. P. K.	Wimbledon, Surrey.	6 5	51° 25' 0" N.	0° 10' 0" W.	2 min.	E. to W.	Shaped like a torpedo; glow like that of an electrically excited vacuum.
21.	Nature, p. 100.	C. J. Taylor.	Wandsworth, Ilford, Essex.	6 4	50° 51' 0" N.	0° 37' 0" E.	Less than 1 min.	20°	2°	E. to W.	

Observations of other beams or arches.

22.	Idem, p. 84.	J. E. Clark.	Lee's	5 25	53 47	0 N.	1 32	0 W.	1 min.	2°	1½°	W.S.W. to E.S.E. E. to W.	Just over the Moon, a green arch. Stationary arc 30° to 40° above horizon and above Moon.
23.	Idem, p. 141.	J. R. Clapham.	Clapham, Lancaster.	5 6	54 7	0 N.	2 22	0 W.		

Foreign observations.

24.	Idem, p. 296.	Prof. Oudemans.	Utrecht	6 23 (local) 6 2 30 G.M.T.	52 6	0 N.	5 6	0 E.	2 min.	3°	E. to W.	Feather-like comet, rose just before Aldebaran, above Saturn, through Pegasus quadrant, and S. of three Eagle Stars. When 90° length attained, separated and division formed 10° × ½°; horizontal direction E. 20° N. to W. 20° S.
25.	Idem	P. Zeeman ...	Zonnemaire (Zierikzee)	6 20 (local).	51 42	0 N.	3 56	0 E.	N. of E. to S.W.	Horizontal bearing E. 20° N. to W. 20° S., through Aldebaran and α Pegasi.
26.	<i>Ciel et Terre</i> , no. 20, p. 465.	Prof. Prignon	Péruwelz (Hainault).	6 30 (local).	50 25	0 N.	3 35	0 E.	1 min.	80°-100°	4°-5°	N.E. to S.W.	"Forme de fuscau." Passed near the zenith.
27.	Idem, p. 466.	M. Thooris.	Bruges	51 12	0 N.	3 13	0 E.	Less than 2 min.	30°-35°	4°-5°	N.E. to N.W.	Like comet's tail. Axis darker than the rest. Across zenith.
28.	Idem	Editor	Bruxelles ...	6 20 (local).	50 51	0 N.	4 20	0 E.	E. to W.	Appeared between α Aurigæ and α Tauri, passed a little N. of zenith, and descended between β and γ Ophiuchi; obscure "noyau."

N.B. Where seconds are not given the latitudes and longitudes are only approximate.

In the foregoing schedule of particulars I have not given the dates of the 'Standard' and 'Times' newspapers; but the letters appeared within a few days of the 17th.

Mr. Munro's letter (obs. 9) was addressed to the Secretary of the Royal Astronomical Society (to whom I am indebted for its loan), and contains a good deal of interesting matter in connexion with the "beam" beyond the particulars abstracted.

A primary question which presents itself in the subject is, whether the "beam" was really and truly a part of the auroral display, or a "meteor," "meteoroid," "cometary body," or something allied to any of these in contradistinction to an auroral beam. Independently of other considerations, two spectroscopic observations which have been recorded put, I think, this point well beyond a doubt. In my own case (obs. 2) I was using the spectroscope (a large Browning direct-vision one) upon the aurora northwards, but, accidentally turning and seeing the beam, I at once applied the instrument to it. The spectrum (not to be found on the adjacent sky) was observed to consist of the well-known principal citron auroral line (W.L. 5569), and a faint greenish-white continuous spectrum extending from about D to F. No other bright line than the principal one was visible; and the continuous spectrum showed no trace whatever of Fraunhofer dark lines—indicating an absence of solar reflected light. The second observation was made by Mr. F. W. Cory, of Buckhurst Hill, Essex, who in a letter to 'Knowledge' (vol. iii. no. 62) says:—"I think there can be no doubt in regard to the connexion between the torpedo-shaped body that was seen on November 17 at 6^h 5^m P.M. and the aurora, as the spectroscopic examination gave the same line for both; and this was situated between D and E in the spectrum, but nearer the former." That the aurora of the 17th was of the same spectroscopic character with previously examined ones is shown by the observations of (1) Mr. Cory, as above mentioned; (2) my own, I finding in the brighter parts the citron line, and a few others indistinctly shown; (3) Mr. T. G. Elger's (obs. 12), who in the pink portions of the aurora found the usual red and green lines; (4) Dr. Wm. Roxburgh, of Bournemouth, who, in a letter to Prof. Piazzzi Smyth, states he found five lines agreeing in position with those given by Prof. Smyth in his plate of the auroral spectrum figured in the 'Royal Observatory of Edinburgh Observations,' vol. xiii.; and (5) Mr. Henry Robinson ('Nature' p. 85), who saw the green line and some others less distinctly in the blue and violet.

Having disposed of this preliminary point, we will now turn to the other incidents of the "beam" detailed in the twenty-six

observations. And, first, (column 4), **time** of its appearance. Owing no doubt to the sudden and unexpected advent of the object, the times recorded are evidently not very certain, and, with the exception perhaps of the Greenwich and some of the foreign observations, are doubtless neither chronometer nor clock time. Many do not profess to be exact; and we are not generally told whether they are Greenwich or local, though probably they are the former. I think, however, they are sufficiently near to enable us to identify observations 1 to 21 and 24 to 28 as all applying to the same phenomenon. For any other purpose the recorded times do not seem of much practical use. Secondly, **time occupied in transit** (7th column). This is also involved in some obscurity. One observer (obs. 12) gives so short a time as "about 6 seconds," while another (obs. 10) extends the time to a length of "less than 4 minutes;" and between these rates we get estimates mostly of 1 and 2 minutes, accompanied by such qualifying expressions as "less," "about," &c. A mean of eighteen observations is found to be 1 minute 15 seconds. As to the longer periods, we know by the experience of counting beats by clock or chronometer that the estimate of time by observers not so accustomed is usually an overrate; and I think we may consider this to have been the case in these instances. Thirdly, as to apparent **length** and **breadth** of the object (columns 8 and 9). These of course, as with the time occupied in transit, would vary with the position of the observer; and at places situated more southward the object would have been seen of greater length and breadth than when seen from the north, while east and west positions would get foreshortened effects. Perspective would come into play with the position of the beam in the sky, most observers agreeing that it lengthened out as it neared the zenith, though one (Mr. Batson, obs. 4) thought it contracted as it passed across the moon. The observations, however, are really not conformable; and though three 30° make their appearance, the observers (obs. 5, 9, 14) differ in position and in their time of transit estimates. The estimated lengths, as might be expected, differ more than the breadths. Perhaps the two most inconsistent records are the Péruwelz (obs. 26) and the Bruges (obs. 27) observations with the same breadth but so great a difference in length. A mean of eleven observations for length (excluding the Rye and Péruwelz observations) and of nine observations for breadth gives $27^\circ \times 3\frac{1}{3}^\circ$ as a general idea of the beam's apparent size.

Column 10 brings us to the **direction of flight**. In this case we have more actually accordant observations, inasmuch as all agree in general terms upon a flight from an east to a

west quarter of the horizon. The exact direction, however, is (doubtless for the same reasons which affected the time of appearance, time of flight, and size) but rarely given. In some instances qualifying words are used, such as (in my own observation) "nearly." In others, degrees of deviation from the cardinal points are given; and generally there seems a tendency on the part of observers situated northwards to place the rising and setting points southwards, and the reverse with the southern observers; but beyond this the deviation from the E. and W. points respectively, according to the observer's station, is not very regularly indicated. The observers have not stated whether astronomical or magnetic points are recorded; probably in most cases the former are intended. The only instances where precise figures are given are those of the observations at Utrecht and Zonnemaire, in which, after stating the direction of flight in a general way as E. to W. and N. of E. to S.W., both observers agree in fixing upon E. 20° N. to W. 20° S. as the horizontal direction. Having thus ascertained as nearly as we may the hour of appearance, time and direction of flight, and size of the beam, its particulars may be summed up as follows:—

- (a) Character, auroral.
- (b) Time of appearance, a little after 6 P.M.
- (c) Time of flight, about 75 seconds.
- (d) Apparent approximate length and breadth, $27^{\circ} \times 31\frac{1}{2}^{\circ}$.
- (e) Direction of flight, magnetic E. to W.

We will now gather from the observers some idea of its general appearance to the eye. The descriptions recorded are sometimes peculiar in expression; but all fairly represent a similar form. Among them are:—"spindle-shaped," "forme de fuseau," "torpedo or weaver's-shuttle," "cigar-ship," "lenticular," and a "comet's-tail," while one observer not inaptly compares it to the well-known fusiform nebula in Andromeda. For descriptive colour, those selected seem to be "white," "pearly white," "greenish white," and "yellow white," it being somewhat significant of the beam's specific character that the last two combined will represent the locus of the principal aurora line indicated in Prof. Piazzzi Smyth's beautiful colour plate in 'Madeira Spectroscopic' by the tint "citron." The *quality* of the light is somewhat variously described in such terms as "glowing," "shining," and "phosphorescent;" and one observer (obs. 20) compared it to an electric glow *in vacuo*; and certainly to me it was not at all unlike the glow of a carbon Geissler tube. The Utrecht observer (obs. 24) speaks of it as "feathery;" but to me and most other English observers the edges were fairly well defined. I thought, how-

ever, I detected a sort of broken and clouded structure, difficult to describe except as being something like what certain forms of cumulo-cirri would appear if illuminated by Balmain paint. Mr. Worthington (obs. 15) noticed it had the ends of a rough splintered appearance. Mr. Batson (obs. 4) says that the object when nearest presented throughout its length (but rather below than above) a remarkable "boiling" appearance (as seeds in a capsule), while the edges appeared smooth and quiet. I did not myself observe this. One curious feature (noticed particularly abroad) was a sort of nucleus, or rather central dulness.

Prof. Oudemans (obs. 24) remarks that when 90° length was obtained the beam separated and a division (longitudinal I presume) was formed $10^\circ \times \frac{1}{2}^\circ$; while in the Bruxelles observation we find "Quand elle eu déjà disparu en partie derrière l'horizon, elle présenta un noyau plus obscur, comme elle l'avait fait à l'horizon oriental." Mr. Munro (obs. 9) is the only English observer who appears to have noticed this feature; and he speaks of a "dark nucleus" about 3° transverse diameter and 2° conjugate—his position, it may be noted, like the foreign ones, being to the east of Greenwich. Mr. Munro further describes the beam as like the edge view of a luminous quoit, its diameter being parallel to the horizon.

This definition is well supported by the circumstances of the nucleus being mostly observed where the beam, as in Belgium, passed overhead, and by the wider breadth (4° to 5°) assigned to it there, at the same time that most English observers saw no nucleus, and estimated, on the whole, the breadth as decidedly smaller. This would give a figure, as seen in the zenith, of a ring much pulled out with a central opening (the nucleus being described as a dark one or a division), while as viewed at an angle the short diameter would be less and the central nucleus lost. The form thus obtained (a pulled-out ring) is what one might anticipate of an elastic gaseous or fluid-like body flying at considerable speed through a resisting medium.

Among the incidents mentioned exceptionally may be noticed Mr. Saxby's (obs. 11) observation of a second beam 70° northward, which appeared as the principal one approached the W. horizon; and also some observations reminding one of the dark-shadow or contrast tint seen near the end of the tail of the recent Great comet. These are as follows:—(1) Mr. Joseph Clark (obs. 10) observed a "dark something" before the "bar" which seemed to indicate the path it would take, and also a dark streak where it passed. (2) Major J. Herschel ('Nature,' p. 87), in quoting from a correspondent's letter to him, says "it left a black cloud of its own shape,

which disappeared in a few seconds." (3) Mr. Dobson (obs. 5) says it was preceded and followed by "a strong black margin." I saw nothing like any of these myself, though I had an impression the beam was following some definite path. Other observations (if made) would be interesting on the point whether these appearances were real or subjective impressions.

Turning our attention now to the combined questions of the object's position, direction of flight, and height above the earth's surface, we already find in print some opinions regarding these. Mr. Saxby (obs. 11) considers:—

1st. The direction must have been S. 70° W., probably $71^{\circ} 45'$, being the complement of magnetic declination.

2nd. The proper motion to have been over a mile a minute.

3rd. The path was vertically over a line on the earth's surface at a least distance from Greenwich of 72 miles, with a height of 44 miles.

4th. The object must have been in the zenith over North Belgium, the Boulogne district, Cherbourg, and the north coasts of Brittany.

This was before the foreign observations were reported; and subsequently Mr. Saxby, after stating that the beam passed in the zenith at Bruxelles (M. Moutigny), and at Laon was seen to northward of the zenith gliding round the upper edge of the great main arch of the aurora, puts the actual elevation "without risk of error" as between 40 and 45 miles. Mr. Taylor (obs. 14), using the observations at York and Woodbridge, deduces a height of 212 miles; and, using those at Hungerford and York, finds 192 miles for height. He considers the beam must have passed overhead in the north of Italy and south of France, and must have been 200 miles in length. Subsequently (upon receipt of the foreign observations) he makes it 70 miles in height when over Belgium, but considers it must have been 150 miles high during the latter part of its course. As to the York observation, it seems desirable to notice that Mr. Taylor does not seem to have much confidence in his own estimate of the beam's apparent distance below the moon; and this estimate is dissented from by a relative who was watching the phenomenon at the same time. Mr. Backhouse ('Nature,' pp. 141 & 315) at first considered the height very considerable; and both he and Mr. Taylor thought Mr. Saxby's estimate of 44 miles too low, probably from near stations being used in the calculations. Later on Mr. Backhouse wrote me he thought 200 miles not an improbable height.

I may here mention that I have not heard of any observations north of York. I made inquiries at Liverpool of the

Astronomical Society recently established there, but could not hear of any ; and it is much to be regretted that one from France is all we hear of from that country.

Upon the foregoing points, Prof. Alexander S. Herschel has very kindly communicated to me, and placed at my disposal, some remarks and calculations to the following effect. He considers that the supplied data (the foreign observations had not then been received) all corroborate each other very fairly, the Hewarth (York) observation excepted, which cannot, he thinks, be usefully employed.

The main body of descriptions serves to track very exactly across the south of England what may be called the shadow-line of the phenomenon thrown by the moon. This is so clearly marked out running magnetic E. and W. from Woodbridge in Suffolk, across Hungerford in Berks, into Devonshire and Cornwall, that the inclination of the beam's path to the geographical meridian (or, rather, to a parallel of latitude) can be assigned almost exactly. But this average result of the observations will of course bear a little variation according to the amount of confidence that we place, and the changes that we think fit to introduce into the interpretation of some of the descriptions. A line drawn subject to such slight variation will be inclined 24° to the geographical east and west line, which is not only a steeper pitch than the actual magnetic variation (about 20° W.) in the part of England where the shadow-path lay, but, *à fortiori*, considerably steeper yet than the real slope of magnetic variation in the region of France (about 17° or 18° W.) where the beam really moved, and was shadowed into England. A line inclined at 18° to E. and W. geographical may, however, be drawn along the central eclipse track of the observations, which will do but little violence to them, and represent them nearly, if not quite, as well as the former line. Whichever shadow-route we adopt, persons placed north (and 10° W.) of it in the direction the moon shone to, would see it *under* the moon at a distance in degrees exactly in proportion to their distance in miles (in that direction) from the shadow-track, or, if placed south of it, *over* the moon in the same way. To ascertain how many miles of displacement in station go to a degree of departure of the object's apparent sky-course above or below the moon, we have only, on the supposition that the same rate of displacement per degree belongs to each of the observations (that is, supposing that all parts of the object's course shadowed by the moon were at one and the same height above the earth, or that its course was really parallel to the earth's surface, or horizontal all the time that it was under observation), to collect all the

observers' distances from the shadow-line, and degrees of "above" or "below" view of the beam's centre relative to the moon's centre as they observed it, into two sums, and then to divide the mileage sum by the degree sum, and the result will be the best average value of the rate that the observations can afford. Exact angular centre differences are only given at Bristol, Guildown, Chatham, St. Ives, and Hewarth (York); and these agreeing very fairly (except the Hewarth one), and the distances being taken from the *first* presumptive track, the result is found to be a total of 280.5 miles for 14.1 degrees of \searrow parallax, or an average rate of 5° to 100 miles. This, projected with a protractor, would make the beam's height 215 miles over Bordeaux, but with a want of probability of such a height and distance. Abandoning therefore the Hewarth observation and accepting the other four, we get in figures as follows:—

Bristol	23 miles	=	1.6
Guildown ...	24.5 "	=	2.0
Chatham ...	33 "	=	2.0
St. Ives.....	37 "	=	5.5
	<u>117.5</u> "	=	<u>11.1</u>
\therefore 100 "		=	9.4

and this gives by projection a height of 92 miles 180 miles south of the shadow-line. Prof. Herschel at this point remarks that, as the result of his experience of the heights of the rayless milky auroral arches, obtained by good parallaxes of them, he has found they constantly occur within a few miles of 100 high, and that possibly the St. Ives observer's parallax may be a little excessive. Taking now a shadow-line inclined 18° to the geographical E. and W. direction (for the moving beam must have lain over a part of France where 18° was about the extent of magnetic variation), measuring the distances again, and preferring this time to use 5° instead of a mean of 5° and 6° as the St. Ives's parallax, we have as follows:—

Bristol	17 miles	=	1.6
Guildown ...	24 "	=	2.0
Chatham ...	27 "	=	2.0
St. Ives.....	41.5 "	=	5.0
	<u>109.5</u> "	=	<u>10.0</u>
\therefore 100 "		=	$9.65 = 9\frac{2}{3}^{\circ}$

Projected with the \searrow 's altitude at Guildown ($28\frac{1}{2}^{\circ}$), these figures will give a height of 100 miles at a distance of 185 miles south of the shadow-line (see Pl. VII. fig. 1); or if, instead of using so large an angle as $9^{\circ}6'$ to protract with, we prefer to

used 100 miles station-distance on each side of the shadow-track so as to get two heights, h^1 and h^2 (see fig. 2), the mean of the two heights obtained, viz. 96 and 173 miles, will be as accurate a result as the observations can be expected to afford, and will = 133 miles.

Upon referring this last projection to Prof. Herschel, after alluding to its being after all only a rough procedure to resort to a triangulation on one side or the other of the moon's apparent altitude with a converging angle so large as 10° to fix the beam's intersection by, and after noticing that, if a 100-mile base must be used, then it were better used on the north side of the shadow-line away from the moon, he then continues:—

Supposing the rate of parallax close to the shadow-line to have been $9\frac{1}{2}^\circ$ per 100 miles (*i.e.* 1° per $\left(\frac{100}{9.5}\right)$ miles = 1° per 10.5 miles), then the best way would be to consider only a small base of 10.5 miles, say, and a small converging angle of 1° corresponding with it (fig. 3), and to calculate h trigonometrically thus:—

$$\left. \begin{array}{l} \text{miles.} \\ x = 10.5 \times \sin 28^\circ \\ \text{and } x = d \times \sin 1^\circ \\ \text{and } h = d \times \sin 28^\circ \end{array} \right\} \therefore \frac{h}{x} = \frac{\sin 28^\circ}{\sin 1^\circ} = \frac{h}{10.5 \times \sin 28^\circ}$$

$$\text{and } \therefore h = \frac{(\sin 28^\circ)^2}{\sin 1^\circ} \times 10.5 \text{ miles.}$$

And then testing the above method's correctness by calculating first for 10.5 miles *north* of the shadow-line, which gives (with fig. 4)

$$\left. \begin{array}{l} \text{miles.} \\ x = 10.5 \times \sin 27^\circ \\ x = d \times \sin 1^\circ \\ h = d \times \sin 28^\circ \end{array} \right\} \text{ or } \frac{h}{x} = \frac{\sin 28^\circ}{\sin 1^\circ} = \frac{h}{10.5 \times \sin 27^\circ}$$

$$\therefore h = \frac{\sin 27^\circ \cdot \sin 28^\circ}{\sin 1^\circ} \times 10.5.$$

And next (by fig. 5) with the 10.5 mile base *south* instead of north of the shadow-line, obtaining

$$\left. \begin{array}{l} \text{miles.} \\ x = 10.5 \times \sin 29^\circ \\ x = d \times \sin 1^\circ \\ h = d \times \sin 28^\circ \end{array} \right\} \text{ and } \therefore h = \frac{\sin 29^\circ \cdot \sin 28^\circ}{\sin 1^\circ} \times 10.5.$$

The mean of these two should be close to

$$h = \frac{\sin 28^\circ \times \sin 28^\circ}{\sin 1^\circ} \times 10.5 \text{ miles, or to } h = \frac{(\sin 28^\circ)^2}{\sin 1^\circ} \times 10.5,$$

as given in the first formula, of which the last two are the criteria to show how far such a mean differs from the use of the rule directly on each side of the shadow-line. The resulting figures are found to be:—

Formula No. 1 $h=132\cdot6$ miles.

„ { „ 2 $h=128\cdot2$ „
 „ { „ 3 $h=136\cdot9$ „

And the mean of Nos. 2 and 3 gives us

$h=132\cdot55$ miles;

in curiously close accord with that obtained by actual projection (fig. 2), viz. **133** miles.

The course the beam pursued would be, at 185 miles from the shadow-line (fig. 1), passing from a little S. of Dresden to Frankfort, then between Bruxelles and Paris, but nearer the latter place and a little N. of it, across Evreux to Quimper; while at 244 miles (fig. 2) we should find it begin at Prague, then to Worms, across Fontainebleau a little S. of Paris, to Nantes. In either case the line would be slightly curved, from its getting into regions where the magnetic variation becomes gradually greater towards the west. As to the speed of motion, a path of between 800 and 900 miles long seems to have been traversed in some 80 or 90 seconds, giving a rate of 10 miles per second.

Prof. Herschel concludes by remarking that he has seen abortive bright streamers move along stationary milk-white auroral bands; and this “shuttle phenomenon” was, he thinks, a streamer-base, or “tendency to shoot up rays,” travelling as a nucleus or concentration of light along an arc or bow otherwise invisible, as streamer-bases, though more faintly visible, frequently do along luminous sharp-edged phosphorescent-looking arches. It will be noticed that this last remark of Prof. Herschel’s well accords with Mr. Joseph Clark’s and my own observations—that the beam seemed to be following a definite and, as it were, “bespoken” track.

It may here, too, be remarked that observers who did not notice the “beam” do mention an arch or arches at about 5^h 30^m, which, except for the former’s flying evanescent character, must have much resembled it.

Two such observations by Mr. Clark and Mr. Clapham (obs. 22 and 23) are comprised in the particulars scheduled. Mr. Clark speaks of a “green” arch, a colour which seems to have been particularly dominant in the 17th of November aurora. Mr. Clapham describes a stationary arc formed of two conical-shaped lights, with apices meeting about the zenith. Dr. Roxburgh, in his letter to Prof. Smyth before

referred to, speaks of a number of arches parallel to the usual one being seen at Bournemouth, one across the zenith which changed in position but slowly. Other observers speak of such arches as distinguished from the usual forms of arch in the north; and the evidence generally tends to the probability that the beam itself may have been the transient lighting-up by a passing glow of an otherwise invisible arc. As regards the beam's apparent course among the stars the observations are not numerous. The Astronomer Royal (obs. 3) speaks of its passing "nearly along a parallel of declination." Mr. Dobson (obs. 5) states that it rose a little south of Aldebaran, slid along at the same N.P.D., and disappeared under Altair. Mr. Worthington (obs. 15) describes it as rising below and to the right of Saturn, but does not trace it further. Mr. Munro (obs. 9) mentions that it rose vertically below the Pleiades. Mr. Saxby (obs. 11) assigns to it a trajectory much flatter than the stars.

For foreign positions we find Prof. Oudemans (obs. 24) at Utrecht giving a precise description, viz. that it rose just above Aldebaran, passed above Saturn, went through the Pegasus quadrant, and sank south of the three Eagle stars. Prof. Zeeman at Zonnemaire (obs. 25) traced it through Aldebaran and α Pegasi. And, lastly, the Editor of *Ciel et Terre* (obs. 28) at Bruxelles says it appeared between ι Aurigæ and α Tauri, and descended between β and χ Ophiuchi. In fig. 7 I have constructed a star-map for the evening in question at Guildown; and on this are traced two dotted lines showing the home and foreign apparent tracks of the "beam." It will be seen that these are in the main conformable to the general tenor of the descriptions, and are in fact portions of circles struck from the equivalent to the magnetic pole as then situate. Circles struck from the magnetic pole on the terrestrial globe will in like manner be found fairly to correspond with Prof. Herschel's assigned paths.

In fig. 6 is found a diagram drawing (with the stars approximately fixed in their apparent places) showing the "beam" as it appeared from Guildown Observatory during the culminating portion of its passage.

Having thus traced the "beam" to be truly part of the aurora, and also having deduced as near as may be from the collected observations the duration and direction of its flight above the earth's surface and its approximate height, it remains to say something of its physical character. This question becomes interesting from the fact that several correspondents to scientific journals and papers have attributed to the phenomenon some sort of "meteoric" character, while

others have, with more precision, claimed it to be a favourable specimen of "cosmic dust" display. The best known and most zealous advocate of this "cosmic dust" theory, which not only touches the character of our "beam" but that of auroral displays in general, is Prof. Groneman, of the Ecole M. de l'Etat at Groningen (Pays-Bas).

In my 'Auroræ' (p. 64) I have briefly referred to the Professor's theory, of which I had only then seen a condensed account; but subsequently he very kindly sent me a full print of his "*Théorie cosmique de l'aurore polaire*" (*Estratto dall' Appendice alle Memorie della Società degli Spettroscopisti Italiani*, 1878, vol. vii.). In chapter ii. ("l'hypothèse fondamentale") of this work, the theory is expounded in connexion with Schiaparelli's discovery of "poudre cosmique" circulating round the central star of our system in elliptic rings, meeting the earth in its orbit, occasioning shooting-stars, meteors, &c., and, amongst other phenomena, luminous apparitions such as auroræ.

This paper is worthy of attentive reading and consideration; for while in its twenty chapters main stress is laid upon the general connexion of the "cosmic dust" with auroræ, with the zodiacal light, and with the "Gegenschein" or antisolar light, the particular phenomena of auroræ are fully and elaborately discussed in detail.

Prof. Groneman has followed up this paper by letters in 'Nature,' vol. xxiii. p. 195, and vol. xxvii. p. 388, and by an article in the same volume, p. 296. The article last referred to is particularly valuable, not only as supplying us with the observations at Utrecht and at Zonnemaire before cited, but as giving a drawing of an auroral arch seen at Groningen, Nov. 2, 1871, resembling the beam, as well as some accounts of other similar phenomena seen during auroræ, and some statistics as to the heights of auroræ, including those of Heis and Flögel, 10 to 100 geographical miles (46 to 461 English miles), Tromholt, 17 geogr. miles, Galle, 40 to 60 geogr. miles, and Prof. Groneman himself, 59 geogr. miles. He considers the course of the beam to have been a great circle cutting the horizon and also the equator in two opposite points. On the cosmic theory we may at once remark that in some respects it is quite possible that auroræ and clouds of meteorites circulating within our atmosphere may resemble one another; but I confess I am not prepared to declare in favour of the "théorie cosmique" as a satisfactory explanation of auroral phenomena. One adverse point urged by Mr. Backhouse ('Nature,' p. 315) is the fact that meteors are mostly given to fly in all directions, while auroral arches and beams (of which this one

is an example) describe courses along parallels of magnetic latitude ; and hence he does not consider its course to have been a meteor-one.

Another divergent point is the rate of motion. The "beam" had, as the result of the observations I have recorded and in Prof. Herschel's opinion, a rate of some 10 miles per second ; while in Guillemin's *Le Ciel*, p. 235, we find the information that the rate of flight of bolides is considered to be 70 to 175 kilometres (40 to 130 English miles) per second.

A third, and to my mind the strongest, objection to the Professor's theory is the well-known spectrum of the aurora. This spectrum is unique of its kind, and has been long since dissociated from that of the zodiacal light (which light Prof. Gröneman connects with auroræ and with the cosmic theory), Prof. Piazzzi Smyth and Pingle having incontestably proved the latter to be a continuous one without any bright lines whatever. Nor does the auroral spectrum at all resemble that of meteors or bolides. The last, according to Konkoly, consists of rays (bright lines) of iron and other metals (probably nickel, cobalt, and manganese, J. R. C.). The aurora-spectrum does not give the idea of a metallic spectrum (with a possible exception of the red and citron lines), but rather that of a gaseous character ; and certainly a positive coincidence of the red and citron lines with any known metal line is far from being proved. Prof. Gröneman has indeed quoted me in his *Théorie Cosmique* (p. 13) as, in *Phil. Mag.* ser. 4, vol. xlix. p. 249 (should be 265), remarking on an "exact concordance" between the iron and aurora spectra. But this is certainly going further than I intended ; for my expression, "*close coincidences*," is too strongly rendered by "*concordance exacte*." If, too, the plate of spectra accompanying that paper be examined, the state of the matter in fact stands thus as to coincidences, thus:—

W.L.	Aurora lines.	W.L.	Iron lines.
6297,	the red line		Not compared.
5569,	the citron line.....	5571,	one of three close
5390,	in blue	5370	[together.
5233,	do.	5231	
5189,	do.	5192	
5004,	do.	
4694,	} do.	a band	} no comparable lines.
4629,			

I have also very considerably qualified any such remark as that above alluded to by pointing out ('Auroræ,' pp. 118–170) that the connexion between the two spectra, though it might be suspected, cannot be considered as proved, the fine iron lines

being so numerous that a coincidence of some of these with the coarser aurora lines may after all be purely accidental. (In 'Photographed Spectra' I have given the spectrum of a meteorite when burnt in the electric arc, which presents several hundred lines in the portion of the spectrum lying between F and H alone.)

In regard to Prof. Groneman's height of auroræ as compared with Prof. Herschel's deductions, it may be desirable to mention two recent authorities on the subject as giving heights very close upon those assigned by Prof. Herschel: viz. Herr Sophus Tromholt, in an article on Auroræ ('Nature,' vol. xxvii. p. 295), speaks of an opinion he has formed as to the height of the aurora, viz. 150 kilometres (90 miles); Baron Nordenskjöld ('Nature,' vol. xxv. pp. 319-321), in the "Scientific Work of the Vega Expedition," Part I. pp. 401-452, speaks of our globe as being adorned with an auroral crown whose inner edge was usually, during the winter of 1878-79, at a height of about 0·03 radius of the earth above its surface (that is, about 115 miles).

While on this part of the subject, it may be useful to refer to the vacuum experiments of Drs. De La Rue and Müller, detailed by them in a paper on the height of the Aurora Borealis read before the Royal Society ('Nature,' vol. xxii. p. 33). Appended to this is a table of deductions from actual observations, showing, amongst other particulars, a scale of miles of auroral heights, and remarks on the character of the electric glow at such heights. 11·58 miles gave a full red glow, 27·42 a carmine, 32·87 a salmon-coloured, 37·67 maximum brilliancy, 81·47 pale and faint, and at 124·15 miles "no discharge could occur." A note appended to the table states "it is conceivable that the aurora may occur at times at an altitude of a few thousand feet"*.

The experiments, however, are open to the remark that the discharge was taken in hydrogen gas and not in air, and that there was an absence of any observation of aurora-like lines or spectrum in connexion with the discharge. Otherwise the experiments would go to show that the auroral discharge generally takes place at a lower level than most observers have ascribed to it, and, if they could be depended on as fulfilling all the conditions of the aurora, would place the beam at a height of 133 miles within the region where no discharge could take place. Possibly, however, the medium in which the discharge was taken might affect the results obtained to the extent of this discrepancy.

* Dr. De La Rue acquaints me he has further experiments in progress.—
J. R. C.

Two other recent matters in connexion with the aurora-spectrum seem to claim an attention before concluding this paper. The first is communicated to 'Knowledge' by Mr. Cory, of Buckhurst Hill (before mentioned). In a letter to that journal (vol. iii. no. 62), dated 30th of November last, after describing the spectrum of the beam, he says:—"Upon a previous occasion, when observing the aurora of October 2, I noticed a bright line in a similar position, and for a few minutes only *three distinct bright lines in the red end of the spectrum*" [the italics are mine]. In a letter to me subsequently, Mr. Cory states he is sure of the lines (in a Browning miniature spectroscope), though he could not give their exact positions—facts which he repeated in a personal interview, adding that by the red end of the spectrum he meant the region on the less-refrangible side of the D lines. Prof. Stokes, in the 'Arctic Manual,' 1875 (p. 26), says "and there are also one or more lines in the red in red auroras." I am not aware of the authorities upon which this statement is made. Although varying W.L. positions have been assigned by different observers to the red line, I have hitherto assumed them to refer to the same line, and did not think more than one had been seen.

With Mr. Cory's uncertainty of positions, it is not easy to make much of his observation; but a circumstance to be noted in connexion with it is, that the single red line generally remarked falls within a group of nitrogen lines, and it would be interesting if the additional lines were found to do so too.

The other matter to be referred to is the recent procuring of an "Artificial Aurora" (as it has been not very happily called) by Prof. Lemström, a name long known in auroral researches.

At present our information on the subject is somewhat meagre. In 'Nature' (vol. xxvii. p. 322) we find that a telegram, dated December 11th last, had been received by the Finnish Academy of Science from Professor Lemström, as chief of the Finnish Meteorological Observatory at Sodankylä. This stated that, having placed a battery with conductors covering an area of 900 square metres on the hill of Orantunturi, he found the cone to be generally surrounded by a halo yellow-white in colour, which faintly but perfectly yielded the spectrum of the aurora. This he considered formed a direct proof of the electrical action of the aurora, and opened a new field in the study of the physical condition of the earth.

A further telegram stated that experiments with the aurora, made December 29th in Enare near Kakala on the hill of Pictarintunturi, confirmed the results of those at Orantunturi. On that date a straight beam of aurora was seen over the galvanic apparatus. It was also stated that, from the magnetic observa-

tions, the terrestrial current ceases below the aurora arc; while the atmospheric current rapidly increases; but depends on the area of the galvanic apparatus, to which it seems proportional.

In a letter to 'Nature' (same volume, p. 389) Prof. Lemström explains that the apparatus was constructed of uncovered copper wire provided at each half metre with fine erected points. The wire was led in slings to the top of the hill, and reposed on the usual telegraph insulators. From one end of this wire was conducted a covered copper wire on insulators to the foot of the hill 600 feet high, which there joined a plate of zinc interred in the earth, and in the circuit was put a galvanometer (no battery is mentioned, J. R. C.). This apparatus produced the halo at Orantunturi and the straight beam at Pictarintunturi as the positive current in the galvanometer at both places. The terrestrial current diminishes (or ceases) below the belt of maxima of the aurora.

In the 'Daily Telegraph' newspaper of March 1st, 1883, Prof. Foerster is stated to have given an account of these experiments, and to have added that the astounding result was the formation of an aurora borealis rising above the mountain-top to an elevation estimated at 300 feet. In a subsequent number of the same paper (March 5th), under a heading "Artificial Aurora," an article is found which alludes in detail to several points in connexion with the phenomena involved, but which to my mind are those really requiring further explanation. One of these is the direct comparison of the electric glow in an exhausted receiver with the auroral discharge. It may be conceded that the two *look* alike; but it is well known that here the comparison ceases, as every effort has been made for years past, without success, to obtain the aurora-spectrum from such a source. Next, the phenomena are attributed to atmospheric electricity; but in point of fact it is strong manifestations of earth-currents, and not of atmospheric electricity, which have generally accompanied auroral displays, notably that of November 17th last. Again, while Prof. Lemström's actual aurora and Planté's artificial auroræ are alluded to in the article in question as connected with the *positive* pole, it was in the glow of the *negative*, or violet pole, that the late Prof. Ångström sought for, and considered he had obtained, an aurora-spectrum.

With regard to the spectrum obtained by Prof. Lemström, if the instrumental resources were adequate for exact measurement, the observation obtained from such a source is doubtless reliable; but it should, by way of precaution, be remembered that even scientists of such calibre as Ångström and Respighi

were on occasions deceived by the presence of a concealed aurora, and thus the zodiacal light and the aurora spectra were at first confounded. Seeing, also, how up to the present time we have quite failed to produce in our laboratories any form of electric discharge which by its spectrum can fairly be pronounced as of an auroral character, it would seem desirable to wait the result of later and fuller particulars from the learned Professor before passing a definite judgment on the matter. We do not at present hear of any comparisons of the "artificial" aurora-spectrum with other spectra which will explain its true and so long hidden character; though doubtless, if we can succeed in establishing an aurora "en permanence," good results may reasonably be expected to follow, one principal cause of failure in explanation of the Aurora mystery being the infrequency of the opportunities afforded of examining its spectrum and comparing it directly with others.

Guildown, April 6, 1883.

XLVIII. *A new Form of Constant-Temperature Bath.* By W. W. J. NICOL, M.A., B.Sc., F.R.S.E., Lecturer on Chemistry, Mason College, Birmingham*.

THE want of a simple and, at the same time, reliable constant-temperature bath has been felt by all who have made specific-gravity determinations. The following apparatus, which has been found thoroughly efficient, was devised by me during the course of my experiments on the specific gravity of salt-solutions, the results of which have been recently published †.

The apparatus consists of three parts—the bath, the heating arrangement, and the thermostat. The bath is of copper, measuring 200 millim. in length, 200 millim. in depth, and 90 millim. in width—a size I found most convenient for use with the Sprengel tubes described in the 'Chemical News' (February 1883). Near one end is soldered a wide brass tube (C, fig. 1) with a slot down one side: this is intended to receive the thermostat and thermometer. The water in the bath is agitated by means of a current of air supplied by a Fletcher's blower. The air escapes from a perforated tube lying on the bottom of the bath.

* Communicated by the Author.

† Proceedings Roy. Soc. Edin. 1881-82; *Ber. deut. Chem. Ges.* 1882, p. 1931; *Phil. Mag.* 1883, February; *Chem. Soc. Journ.* 1883, March.

The heating arrangement is as follows :—At the end of the bath, where the tube C is placed, two holes are made close to

Fig. 1.

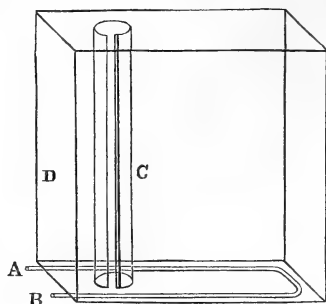
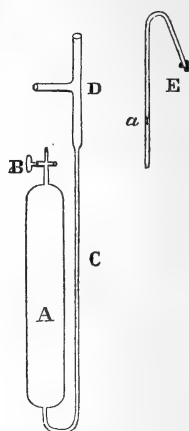


Fig. 2.



the bottom ; through these passes the copper tube A, B. The end A is coiled into a spiral of four or five turns about an inch in diameter (not shown in the figure). The bath having been placed on a stand in the required position, with the end D away from the experimenter, the copper spiral is surrounded by a roll of tin-plate, and the upper end of the tube connected with the water-supply, which must not be direct from the main. The end B is then furnished with a tube to convey away the water. Below the spiral is placed a Bunsen, the supply of gas to which is governed by the thermostat.

The thermostat is shown in fig. 2. The reservoir A is made of thin glass tube, 30 millim. wide and about 170 millim. long; to the upper end is joined a small stopcock B; to the lower end the tube C (4 millim. internal diameter) is connected (the length of C is about 230 millim.); to the upper end of C is joined a T-piece D made of a tube 6 millim. in internal diameter and 80 millim. long. Finally a thin tube E, 2·5 millim. in external diameter, is bent in the form shown—the longer limb slightly drawn out, and a small hole made at *a*. The reservoir A and tube C are then filled with benzoline or paraffin oil, according to the temperature required for the bath; and a quantity of mercury is introduced sufficient to occupy the whole of the tube C and a small part of A when the desired temperature is attained. The tube E is then inserted into D and fastened by a small cork, in such a way that the end is just below the point of juncture of C and D.

The thermostat is next placed in position in the tube C (fig. 1), the thermometer placed beside it, and the bath filled up with water; the air current is then turned on, and the gas-supply so arranged that it enters the thermostat by E and passes to the Bunsen by the side tube of D. A very slow stream of water is allowed to flow through the spiral, and the Bunsen is lighted. The stopcock B, at first closed, is opened from time to time, until, when the desired temperature is reached, the column of mercury in C just touches the end of E; the flame of the Bunsen is thus lowered, and cold water flows through the spiral cooling the bath. As the temperature of the bath falls, the mercury falls below the end of E, and the water is heated before entering the bath; after a few oscillations of temperature a state of equilibrium is reached, in which the size of the flame is so adjusted to the quantity of water flowing through the spiral that the temperature of the bath remains constant.

I have found that the extreme variation of temperature does not exceed $0^{\circ}\cdot 05$ for a temperature of 20° C., and that on lighting the gas in the morning the constant temperature was attained in less than an hour, and never varied from day to day.

XLIX. "Rotational Coefficients" of various Metals.

By EDWIN H. HALL*.

THE experiments described below were made at the laboratory of Harvard College during the summer of 1882; and most of the results obtained were given at the Montreal meeting of the American Association.

At the York meeting of the British Association, September 1881, I gave a list of certain metals with an approximate value of the "rotational coefficient"† for each, as determined by my experiments. This list was published in the Report of the Association. Several of these metals had, however, been examined in an extremely inaccurate manner, as was stated at the time, and the numbers assigned them were marked as doubtful. Thus a part of the list ran:—

Name of metal.	Rotational coefficient, arbitrary scale.
Zinc	+15 ?
Aluminium	—50 ?
Magnesium	—50 ?
Copper	—10 ?
Brass	— 1·3 ?
Lead	No effect discovered.

* From Silliman's American Journal for March 1883.

† Phil. Mag. Sept. 1881, p. 162.

Repeating, still in a hasty and rough manner but more carefully than before, the experiments with all these metals except magnesium, and using indeed the same pieces of metal as before, I found:—

Name of metal.	Rotational coefficient.
Zinc	+ 10·5
Aluminium	— 37
Copper	— 6·5
Brass	— 1·4
Lead	No effect discovered.

It will be observed that the value obtained for brass, which is small, is but little changed; but those for zinc, aluminium, and copper have each been reduced about 25 or 30 per cent. We may perhaps by analogy, without actual determination, write

Magnesium . . . — 35

All these values may still be subject to errors of 10 or 20 per cent., but will nevertheless serve present purposes tolerably well if substituted for those given in the list previously published. Such a list, though rough, may be compared with other lists in which the same metals are arranged relatively to various physical properties; and any analogies thus suggested may be tested further by more accurate and detailed investigations. In fact, to go no further than the above table itself, the fact that the small rotational coefficient in brass lies between the positive coefficient in zinc and the negative in copper suggests the advisability of a careful study of the transverse effect in alloys.

In the *Philosophical Magazine* for September 1881 I stated that the transverse current obtained with a nickel strip is much increased, other conditions remaining unchanged, by rise of temperature. It was a question of much interest whether the transverse current in the non-magnetic metals would prove to be affected in a similar manner. It might be found indeed that the transverse current would increase at the same rate as the electrical resistance, in which case it would appear that the transverse effect depends upon the rate of fall of electric potential along the strip of metal rather than upon the strength of the direct current.

Accordingly, from a sheet of No. 2 gold foil*, the thinnest foil used by dentists, a piece was cut in the form of a Greek cross. The extremity of each arm of this cross was soldered to a disk of brass. The four brass disks were screwed to a

* "Standard," R. S. Williams and Co.

plate of hard rubber in such a manner as to extend the arms of the cross, which was then fastened to the rubber plate by means of melted resin run between. Wires soldered to the disks served for the connexions.

The very considerable difficulty of soldering so thin a strip of gold and then fastening it upon the plate has heretofore prevented my employing this method of making connexions with that metal, screw-clamps being used instead. The practical advantage of soldered connexions is of course considerable, though not so great as might at first appear. Resin, although very brittle, was used as a cement, for the reason that within the limits of temperature to be employed it is more rigid than any other cement I could hit upon, and therefore less liable to allow the gold strip to become distorted or strained by the stress it is subjected to while carrying a current across a powerful magnetic field.

In order to control the temperature of the gold strip, it was placed in a narrow tank between the poles of the electromagnet, and water was made to flow slowly through the tank, from bottom to top, during the experiment. The lowest temperature used was about 2° C., the highest about 30° C., as will be seen below.

August 2, the following results were obtained in the order given :—

Gold.	
Temperature.	Numbers proportional to transverse effect.
30.2° C.	1738
2.2	1703
2.6	1748
30.0	1746

No attempt was made to determine the absolute magnitude of the rotational coefficient in this specimen of gold ; so that the numbers given in the second column must not be used for comparison with numbers elsewhere given as proportional to the rotational coefficient in gold or other metals.

The magnetic field in these experiments had an intensity of about 1900 C.G.S. The primary current was not measured in absolute units. It was such as a Bunsen cell yields in a circuit of a few ohms resistance.

From the above table we get:—

Temperature.	Numbers proportional to transverse effect.
30.2° } 30.1°	1738 } 1742
30.0° }	1746 } 1742
2.2 } 2.4	1703 } 1726
2.6 }	1748 } 1726

The mean of the numbers at low temperature is therefore less than the mean for the higher temperature by rather less than one per cent. The particularly small number 1703, which causes this result, was obtained from a rather bad series of observations, and is probably entitled to less weight than the others. In fact, though the numbers as they stand seem to indicate a decrease of about one per cent. in the value of the transverse effect for a fall of about 30° C., I think it better to say that we have here detected no certain effect of fall of temperature.

It is evident from these experiments that, if the value of the transverse effect in gold varies at all with change of temperature, it varies far less than the electrical conductivity. We must conclude therefore, that this effect depends rather upon the magnitude of the current through the gold than upon the fall of potential per unit of length. This conclusion was long ago reached* ; but doubt had been cast upon its correctness by the experiments upon nickel above mentioned.

Turning again to the magnetic metals and taking a strip of thin iron, experiments were made similar to those with gold as above described. The results obtained, though by no means so accurate as those obtained with gold, show that in iron the rotational coefficient is very strongly affected by change of temperature, the effect being an increase of perhaps two thirds of one per cent. for a rise of 1° C. Possibly a comparison of the effect of change of temperature upon the magnetic permeability of iron, nickel, and cobalt with the effect of the same change upon the rotational coefficient, will be of value when both effects shall have been more fully studied.

Leaving the matter of effect of change of temperature and referring again to the article on nickel and cobalt (Phil. Mag. Sept. 1881), we see that the rotational coefficient in nickel decreases as we increase the strength of the magnetic field ; *i. e.* the rotational *effect*, other things being equal, increases less rapidly than the intensity of the magnetic field.

Experiments were made for the purpose of determining whether a similar relation would hold in iron. The iron was tested in magnetic fields varying in intensity from about 1000 to about 7500 in absolute C.G.S. units. Judging from the behaviour of the strip of nickel previously examined in this manner, the R. C. of that metal would be about 20 per cent. greater in a field of intensity 1000 than in one of intensity 7500. For certain reasons I do not feel perfect confidence in

* Silliman's American Journal, September 1880.

the numerical results obtained with iron, and do not consider them worth publishing. To myself, however, they make it seem probable that the rotational coefficient in iron is *less* in a field of intensity 1000 than in a field of intensity 7500; and I expect to prove this when I am able to take up the matter again. Cobalt should of course be examined in the same way; nor must it be forgotten that it is by no means proved, as yet, that the non-magnetic metals will show a constant rotational coefficient when tested between wide limits of magnetic force.

The object of another experiment was to determine, if possible, whether any part of the rotational effect could be made permanent under favourable conditions. For this purpose a piece of clock-spring was taken, tempered very hard, and then reduced by action of nitric acid to a thickness of about $\cdot 06$ millim. This piece of steel was firmly imbedded upon a plate of glass in a layer of cement made of melted bees-wax and resin. This plate, with the usual electrical connexions, and with a current flowing through it, was placed in the usual position between the poles of the electromagnet; the magnet current was turned on, then off, and the plate removed from between the poles in order to avoid the action of the very considerable residual magnetism of the electromagnet. A reading of the Thomson galvanometer in the transverse circuit was now made; then the plate was replaced between the poles and the current turned on again, but in the opposite direction. The magnet-current being again interrupted, the plate was again removed from the field, and another reading of the Thomson galvanometer was made. The two readings differed by several centimetres on the galvanometer-scale. The experiment was repeated, and always with a like result.

There was no room for doubt that the direction of the equipotential lines in the steel was permanently changed by the action of the magnet. This change was in the same direction as the temporary change produced by the magnet's action, and perhaps equal to 2 per cent. of the temporary change.

This result was of course not unexpected. The hardened steel must have become permanently magnetized transversely; and this magnetization should produce an effect similar to that of temporary magnetization. The experiment is of interest, however, as indicating that the rotational effect is not due to the mere mechanical stress to which the metal is subjected in the magnetic field; for though no one has ever pointed out how any such stress could produce the effect observed, many have no doubt questioned whether it might not after all be due in some obscure way to such stress.

It may be stated incidentally that the transverse effect

appears to be much greater in steel of blue temper than in soft iron, and, again, much greater in steel of very hard temper than in steel of blue temper. If we call the effect in soft iron 1, the effect in blue steel is perhaps 2, and that in very hard steel 4.

L. *The Resistance of the Electric Arc.*

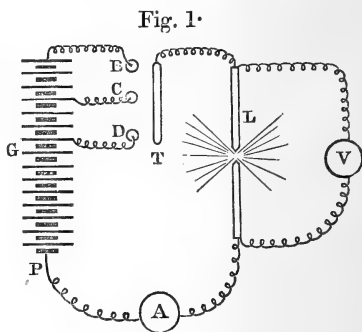
By Professors W. E. AYRTON, *F.R.S.*, and JOHN PERRY, *M.E.**

ONE of the results of the elaborate set of experiments on the Electric Light conducted in 1878 by the late Mr. Schwendler was the conclusion to which he came, that the supposition that the resistance of an arc of constant length was inversely proportional to the current which passed through it was highly probable. His experiments, however, were not sufficient to absolutely determine this point; and it has therefore appeared to us important to obtain further information on the subject, which, with the assistance of the students working in our laboratory, we have from time to time done with the following results.

1. The method employed by us in the first instance was as follows:—A number of Grove cells, G, were arranged in series (fig. 1); and one pole

P was connected through an ammeter A with one carbon of the electric light L, the other carbon of which was attached to a mercury trough T, which, by means of a metallic bridge-piece, could be connected with any one of the mercury-cups B, C, D, each of which was permanently electrically connected with the terminal of a different number of cells.

The two carbons were also connected with the terminals of a voltmeter V, by means of which the difference of potentials between the carbons at any moment could be determined. The experiment was made thus:—The bridge-piece was put into D, and the carbons by means of a rack adjustment separated until a good steady light was obtained, when readings of the ammeter and voltmeter were taken. A second bridge-piece was now put into C and that in D quickly withdrawn, the effect being to suddenly increase



* Communicated by the Physical Society of London; read December 9, 1882.

the number of cells in circuit producing the light, increasing therefore the current without interrupting it, and without changing the distance between the carbons, as the lamp had no automatic adjusting arrangement. Readings of A and V were then quickly taken and the operation reversed—that is, the bridge-piece put into D and that in C quickly withdrawn, which had the effect of again reducing the current; and if the change back again were effected not long after the first, the carbons were not sufficiently burnt away by the stronger current to make the light go out when the current was reduced, so that a third set of readings of A and V could be taken. In this way, for the same distance between the carbons two readings of the lower current and its corresponding carbon difference of potentials, and one intermediate reading of the higher current with its carbon difference of potentials, were obtained. The whole experiment was now repeated with the cells P C and P B instead of P D and P C. The following is a sample of the results obtained from a number of tests with 30, 40 and 50 Grove cells :—

Number of cells.	Current, in ampères.	Difference of potentials between carbon, in volts.	Work in foot-pounds per second in arc.
30	6.52	30.4	146.2
40	10.16	30.4	227.8
50	11.92	30.4	267.2

The last current is therefore nearly double the first, but the difference of potentials between the carbons is not materially altered by the increase of the current and the light.

Subsequently a large number of experiments were made using a Brush dynamo in place of the Grove cells, and increasing and diminishing the current by suddenly increasing and diminishing the resistance in circuit without stopping the current.

In the earlier experiments for each current its value was read on the ammeter as well as the difference of potentials between the carbons on the voltmeter; but since even with a very dead-beat ammeter some little time must elapse when the currents are alternately doubled and halved by taking out and inserting resistance in circuit, and since even with a slight delay the stronger current burns away the carbon points very rapidly, and so makes the distance between them for the stronger currents greater than for the weaker, it was thought better in the later experiments merely to take readings of the voltmeter when the resistance was altered backwards and forwards sufficiently to alternately treble and diminish to one third the current as shown by the earlier ex-

periments. The following are samples of the readings of the voltmeter, the distance between the carbons in each case being fixed and the current alternately trebled and diminished to one third.

Current approximately.		Difference of potentials between carbons, in volts.	
1	26·5	
3	26·5	
1	24·5	
3	25·5	
1	26·5	Distance between carbons re-adjusted.
3	26·5	
1	25·5	
3	26·5	
3	32	A somewhat greater distance between the carbons.
1	30	
3	39·5	
1	30	
3	34	
1	34	
3	36	
1	38	
3	30	
1	28	
3	30	
3	30	Distance between carbons re-adjusted.
1	28	
3	30	
1	28	
3	30	

It would appear therefore that for a fixed distance between the carbons the difference of potentials necessary to maintain the arc is nearly but not quite independent of the current, the electromotive force requiring to be slightly increased when the current is very much increased.

2. The second part of the investigation was for the purpose of ascertaining in what way the differences of potentials between the carbons varied with the length of the arc when the current was kept constant. For this purpose the arc was projected on a distant scale by means of a lens, the magnifying-power of the arrangement being calculated, first, by comparing the distance between the scale and the lens with the distance between the arc and the lens, secondly by putting close to the arc a piece of carbon of known thickness and measuring

quickly the thickness of its image as projected on the distant screen, before the piece of carbon had time to burn.

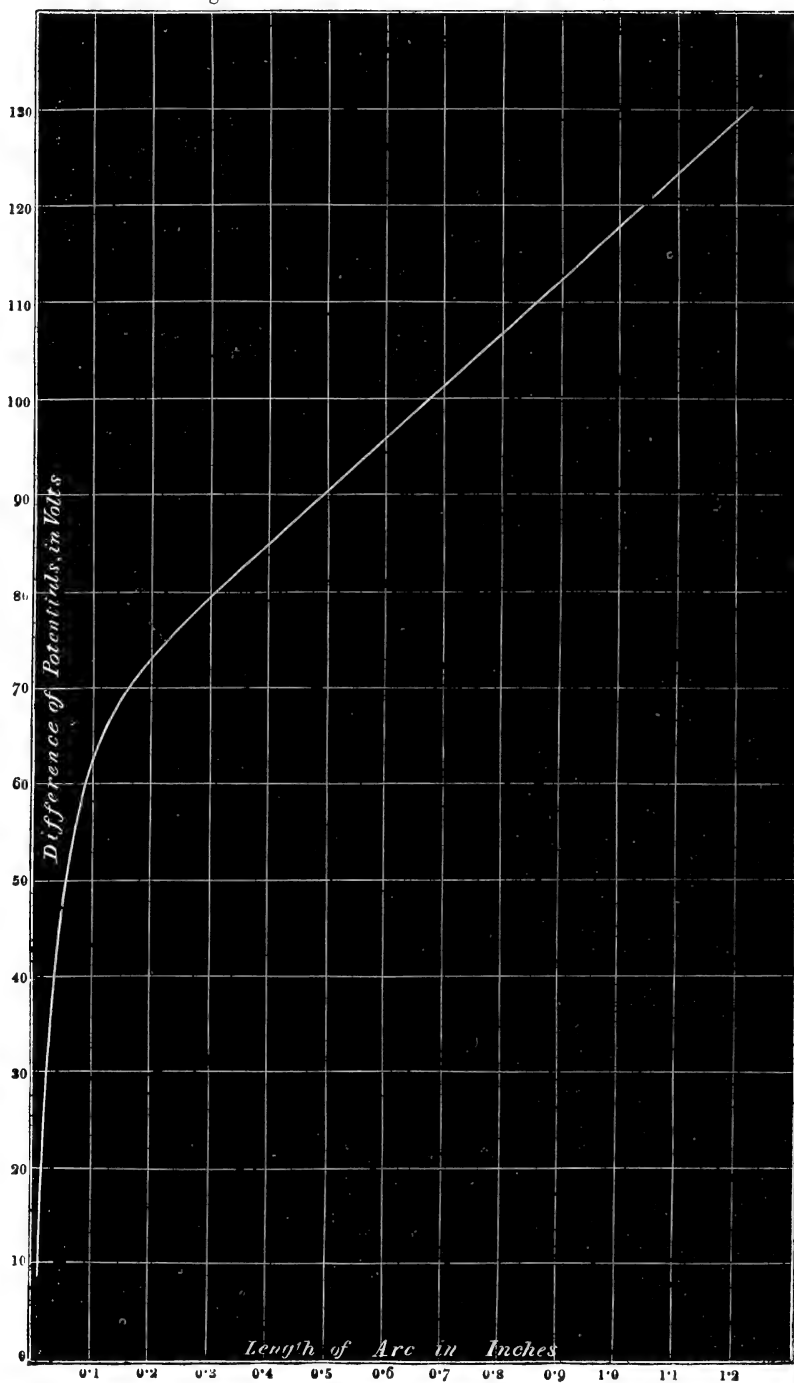
For each set of experiments a particular current was decided on: the carbons were put successively at different distances apart and the resistance in the circuit varied until the prearranged current was flowing through the arc, when instantly the actual projected distance between the carbons on the screen was read off and the difference of potentials between the carbons in volts; or the resistance in the circuit external to the lamp could be left fixed, and the carbons gradually withdrawn until the prearranged current was flowing through the arc, when, as before, the projected length of arc and the difference of potentials between the carbons was read off. A large number of experiments were made in this way with a Brush machine for currents varying between 5.5 and 10.4 ampères, the distances between the carbon points from 0 to one and a quarter inch, and the difference of potential varying from 0 to 140 volts, the carbons being 0.24 inch thick. The result when plotted gave a curve similar to that shown in fig. 2 (p. 350), horizontal distances representing distances between the carbon points, and vertical distances the difference of potentials between the carbons. For all the currents approximately the same curve was obtained—a result to be expected, seeing that the first investigation showed that the difference of potentials between two carbons necessary to produce an arc depended almost entirely on the distance between them, and hardly at all on the strength of the current. The equation to the curve we find to be approximately as follows:—

$$E = 63 + 55a - 63 \times 10^{-10}a,$$

where E is the difference of potentials in volts between the carbons, and a the distance between their points in inches. It will be seen that at first the difference of potentials necessary to maintain the arc increases rapidly with the distance, and that at a distance of about one tenth of an inch it is about 60 volts. From this the curve bends rapidly up to a point corresponding with a distance between the carbons of about one quarter of an inch; and for greater distances between the carbons than one quarter of an inch, the increase of difference of potentials becomes nearly proportional to the increase of distance, being about 54 volts per inch increase.

This law is very like that found by Mr. C. F. Varley for the discharge through a vacuum-tube, which was that the current was proportional to the difference of potentials minus a constant; for this is equivalent to saying that, *cæteris paribus*, the difference of potentials necessary to produce a fixed

Fig. 2.—The Resistance of the Electric Arc.



current is proportional to the length of the tube plus a constant. The curve we have obtained is also strikingly like that obtained by Drs. W. De La Rue and Hugo Müller for the connexion between the electromotive force and the distance across which it would send a spark*. These gentlemen also made experiments on the electric arc with their large battery ; but we do not find recorded any results with carbon points. On page 185 of the reprint from the 'Philosophical Transactions' of the account of their researches, the result of an experiment in air between two brass points is given ; but according to that, when the arc was half an inch in length the difference of potentials between the brass points was that of 657 of their cells, or about 700 volts. How far the very high electromotive force found by Drs. W. De La Rue and Hugo Müller, to be necessary in this case, arose from a combination of the material employed for the electrodes and the smallness of the diameter of the brass electrodes, or whether the law that "the electromotive force necessary to maintain an arc depends mainly on the length of the arc and hardly at all on the strength of the current" fails when the current is below a certain small limit, we are unable to say ; but of course both the diameter of the brass electrodes they employed and the strength of the current that was passing (0.025 ampère) in the arc was very much less than that used in any ordinary electric light, and to which the experiments of Mr. Schwendler and ourselves especially refer. It is very probable that the difference in the material of the electrodes has mainly to do with the difference between their results and ours ; and we think it very probable that with very soft carbons an arc of a given length could be maintained with a much less difference of potentials than that found by us, since it would be more easy for a shower of carbon particles to be maintained between the ends of the carbons.

We have used as the title of this short communication the Resistance of the Electric Arc ; but we are perfectly aware of the objections to this expression. How far the opposition to the passage of the currents in an electric arc is due to pure resistance, and how far to an opposing electromotive force, is up to the present time by no means certain. That there is some opposing electromotive force, seeing that mechanical disintegration of the carbon and transporting of its particles occurs, is, as was pointed out some years ago by Edlund, almost certain ; but seeing that this opposing electromotive force ceases to exist with the extinction of the arc, and

* Page 32 of the Reprint of "Experimental Researches on the Electric Discharge," Phil. Trans. part i. vol. 169.

probably varies as the pure resistance varies, and further remembering that an opposing electromotive force which has no existence apart from a combined resistance acts in any electrical test exactly as a resistance, it must be always very difficult experimentally to separate them. All of course that we can measure electrically is the difference of potential between the carbons and the current passing between them; and this is what we have been measuring all through these two investigations.

It may be here noted that in all probability the conduction from particle to particle in a microphone is of the nature of a small electric arc, or, rather, perhaps a convective discharge, seeing that the resistance in a microphone varies with the current used to measure it; indeed it is probable, when the pieces of carbon or other material employed, are so pressed together that close intimacy of contact of the particles makes the resistance tolerably independent of the current, that then the pieces of carbon will not act as a microphone at all.

We have to thank Messrs. W. Atkinson and Lt. B. Atkinson, two of our students, for much assistance rendered us in these experiments.

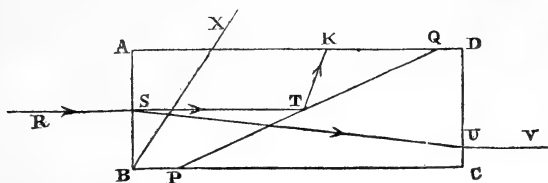
II. *On Polarizing Prisms.* By R. T. GLAZEBROOK, M.A., F.R.S., Fellow and Lecturer of Trinity College, Demonstrator in the Cavendish Laboratory, Cambridge*.

IN a paper on Nicol's prism (Phil. Mag. vol. x. 1880) I have considered some of the defects of Nicol's prism as a means of producing plane-polarized light. In the present paper I propose to describe a form of polarizing prism free from most of these. For many purposes, one of the great objections to Nicol's prism is the lateral displacement produced by it in the image of an object viewed through it. If we place a Nicol before the object-glass of a telescope, on turning the Nicol round its axis the image moves across the field. This has been remedied somewhat by cutting prisms with their ends at right angles to their length, and making the angle between the normal to the face on which the incident light falls and the plane of Canada balsam such that the ordinary ray is totally reflected there while the extraordinary ray passes through. But this is not entirely successful; for let $ABCD$ (fig. 1) be a section of such a prism by a plane parallel to the edge BC and at right angles to the Canada balsam.

* Communicated by the Physical Society of London; read 14th April, 1883.

Let PQ be the trace of the balsam. In an ordinary Nicol's prism AB would be inclined at about 74° to AD , and PQ

Fig. 1.



would be at right angles to AB , AD and BC being parallel to edges of the rhomb of spar. In the case now being considered, AB is at right angles to BC , BC being still parallel to a rhombic edge.

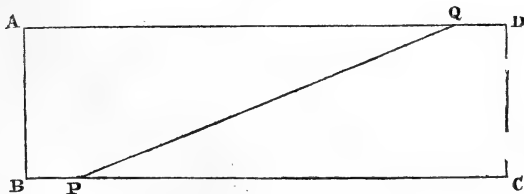
Consider a ray RS incident normally on AB . The ordinary ray ST enters the spar without deviation, but is reflected by the balsam at T in direction TK ; the extraordinary ray is refracted at the face AB in direction TU , and turned from its original path in virtue of the extraordinary refraction. It emerges along UV parallel to its original direction, but displaced to one side, so that the extraordinary image of the object seen is displaced to one side by the passage of the light through the spar.

In the prism considered in fig. 1, the optic axis lies in the plane of the paper, making an angle of $57^\circ 30'$ with BC .

Suppose now we cut a rectangular parallelepipedon from a piece of spar, in such a way that two of its faces are at right angles to the optic axis while the other four are parallel to it.

Let $ABCD$ (fig. 2) be a section of the solid by a plane also at right angles to the optic axis, and therefore parallel to two faces and at right angles to the other four; and suppose that

Fig. 2.

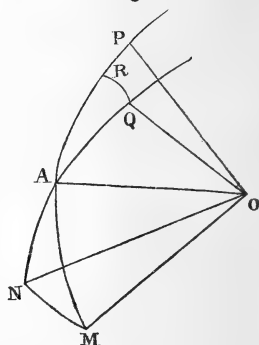


BC is about three times AB . Let PQ be inclined at about 20° to BC , and suppose the prism cut in two by a plane at right angles to the paper and passing through to PQ . Then let the faces of section be polished, and cemented together with Canada basam. The optic axis will be at right

angles to the plane of the paper, and the section of the wave-surface by that plane will be two circles of radii A and C , these being the ordinary and extraordinary wave-velocities respectively. Hence a ray falling on the face AB in any direction in the plane of the paper will be divided into two, which will both undergo ordinary refraction, so that if the incident ray be normal to the face AB , the extraordinary and ordinary rays in the prism will coincide in direction, both being normal to the same face. The extraordinary ray is not deviated by the refraction; so that no lateral displacement of the extraordinary image is produced by the prism. The ordinary ray is incident at about 70° on the face PQ ; it is therefore totally reflected, and the emergent light is plane-polarized. The prism differs from one described by Prof. S. P. Thompson (*Phil. Mag.* Nov. 1881) only in the fact that its ends are normal to its length instead of being inclined obliquely to it. But this form of prism has other and more important advantages.

Let OM , ON (fig. 3) be two extraordinary wave-normals, and OA the optic axis. Pass a plane MOA through OM and OA , and in this plane draw OP at right angles to OM ; then OP is the direction of vibration in the wave which travels along OM . Similarly, if NOA be a plane through ON and OA , and OQ a line in it at right angles to ON , OQ is the direction of vibration for the wave along ON ; and it may happen, clearly, that OP and OQ are inclined to one another at a large angle even when OM and ON are close together. Suppose, then, that the extraordinary pencil of wave-normals which is traversing the spar is slightly conical, and that ON , OM are two of the wave-normals; the planes of polarization are inclined to each other at an angle equal to POQ ; and this may be considerable. Or, again, suppose that we have a polarized pencil of parallel wave-normals incident on the prism. We determine the position of their plane of polarization by turning the prism until no light passes through. Suppose that, when this is the case, the incident light is parallel to OM . Now let the plane of polarization of the incident light be rotated, and suppose we wish to measure this rotation; we turn the prism until the light is again quenched. Theoretically the axis round which the prism has been turned should be

Fig. 3.



parallel to OM . In practice it is difficult to ensure this; and in general the direction of the wave-normal relatively to the optic axis will be changed, and may now be ON say. But since the planes of polarization of the waves along OM and ON are different, the angle through which the prism has been turned will not be the angle through which the plane of polarization of the incident light has moved.

Now Nicol's prism is so cut that the angle between the planes of polarization of two waves inclined to each other at but a small angle as they traverse the crystal is considerable. If, then, a slightly conical pencil traverse the prism, the angles between the planes of polarization of the different waves are considerable; or if a parallel pencil traverse the prism inclined at but a small angle to the axis of rotation, and the plane of polarization of this beam be rotated, that rotation will differ considerably from the angle through which the prism has to be turned to reestablish blackness.

In our figure the wave along OM is polarized in a plane at right angles to OP , that along ON in a plane at right angles to OQ . Consider now a conical pencil of wave-normals in air: it is clearly impossible for it to be plane-polarized, if by plane polarization we mean that the directions of vibration are parallel to the same line; for we cannot have a series of lines touching a sphere all parallel to the same line. Such a pencil, however, may be said to be most nearly plane-polarized when all the directions of vibration are parallel to the same plane; and this plane will be that which passes through the axis of the pencil and the direction of vibration for the wave-normal which coincides with the axis. For if this be the case, the whole of the pencil can pass unaltered either as an ordinary or extraordinary wave through a piece of spar on which its axis falls normally, provided that the optic axis of the spar be respectively either at right angles to or parallel to the plane in question. Using "plane polarization" in this sense, we proceed to consider when a conical pencil of given vertical angle travelling in a piece of uniaxal crystal is most nearly plane-polarized.

Now let OM (fig. 3) be the axis of the pencil, and OP the direction of vibration for the light travelling along OM , and let ON be any other wave-normal. According to the above statement, the conical pencil will be most nearly plane-polarized if the vibration travelling along ON is parallel to the plane POM . If, however, the pencil be travelling in a crystal, it is clearly impossible in general for the displacement along ON to be parallel to this plane. For let OA be the optic axis; OA lies in the plane MOP . Pass a plane through

O A, O N, and in it draw O Q at right angles to O N; O Q is the direction of displacement which travels along O N, and O Q is not parallel to the plane P O M.

We can resolve the displacement along O Q into two, in and perpendicular to the plane P O M. The light then will be most nearly plane-polarized when the average intensity of the vibrations normal to this plane is least; and it remains to find the condition for this.

In fig. 3 let QR be perpendicular to AP. Let $AM = \alpha$, $NM = \beta$, $AN = \theta$, $AMN = \phi$. Let ρ be the amplitude of the displacement along OQ. The displacement normal to the plane PAM is $\rho \sin QR$, and the intensity of the wave is proportional to $\rho^2 \sin^2 QR$.

We are to consider a hollow conical pencil with OM as axis. An element of such a pencil at N will be $\sin \beta d\phi$; and the total energy in the pencil, so far as it depends on the displacement normal to the plane PAM, is

$$2 \int_0^\pi \rho^2 \sin \beta \sin^2 QR d\phi.$$

Now

$$AQ = \frac{\pi}{2} - \theta,$$

$$\begin{aligned} \sin QK &= \sin AQ \sin RAQ \\ &= \cos \theta \sin NAM, \end{aligned}$$

$$\sin NAM = \frac{\sin \phi \sin \beta}{\sin \theta};$$

$$\therefore \sin QR = \cot \theta \sin \phi \sin \beta. \quad \dots (1)$$

$$\text{Also} \quad \cos \theta = \cos \alpha \cos \beta + \sin \alpha \sin \beta \cos \phi. \quad \dots (2)$$

Substituting in the value of $\sin QR$, we find for the energy required the expression

$$2\rho^2 \sin^3 \beta \int_0^\pi \frac{\sin^2 \phi (\cos \alpha \cos \beta + \sin \alpha \sin \beta \cos \phi)^2 d\phi}{1 - (\cos \alpha \cos \beta + \sin \alpha \sin \beta \cos \phi)^2}. \quad \dots (3)$$

And we require to evaluate this integral.

Let

$$\cos \alpha \cos \beta = a, \quad \sin \alpha \sin \beta = b.$$

Then

$$\begin{aligned} & \int_0^\pi \frac{\sin^2 \phi (\cos \alpha \cos \beta + \sin \alpha \sin \beta \cos \phi)^2 d\phi}{1 - (\cos \alpha \cos \beta + \sin \alpha \sin \beta \cos \phi)^2} \\ &= \int_0^\pi \frac{\sin^2 \phi (a + b \cos \phi)^2 d\phi}{1 - (a + b \cos \phi)^2} \\ &= \int_0^\pi \frac{\sin^2 \phi}{1 - (a + b \cos \phi)^2} d\phi - \int_0^\pi \sin^2 \phi d\phi. \end{aligned}$$

The first term

$$= \frac{1}{2} \int_0^\pi \left\{ \frac{\sin^2 \phi}{1 - (a + b \cos \phi)} + \frac{\sin^2 \phi}{1 + (a + b \cos \phi)} \right\} d\phi.$$

But

$$\begin{aligned} \int_0^\pi \frac{\sin^2 \phi d\phi}{c + d \cos \phi} &= \int_0^\pi \frac{(1 - \cos^2 \phi) d\phi}{c + d \cos \phi} \\ &= \int_0^\pi \left\{ \frac{c}{d^2} - \frac{\cos \phi}{d} - \frac{c^2 - d^2}{d^2 (c + d \cos \phi)} \right\} d\phi \\ &= \frac{\pi c}{d^2} - \frac{c^2 - d^2}{d^2} \frac{2}{\sqrt{c^2 - d^2}} \frac{\pi}{2}, \\ &\quad \text{if } c \text{ is } > d, \\ &= \frac{\pi}{d^2} \{c - \sqrt{c^2 - d^2}\}. \end{aligned}$$

Hence

$$\int_0^\pi \frac{\sin^2 \phi}{1 - a - b \cos \phi} d\phi = \frac{\pi}{b^2} \{1 - a - \sqrt{(1 - 2a + a^2 - b^2)}\}; \quad (4)$$

for we can easily show that c is $> d$ in this case. And

$$\int_0^\pi \frac{\sin^2 \phi}{1 + a + b \cos \phi} d\phi = \frac{\pi}{b^2} \{1 + a - \sqrt{(1 + 2a + a^2 - b^2)}\}. \quad (5)$$

And the required integral is

$$\frac{\pi}{2b^2} \{2 - \sqrt{(1 - 2a + a^2 - b^2)} - \sqrt{(1 + 2a + a^2 - b^2)} - b^2\}. \quad (6)$$

But

$$a^2 - b^2 = \cos^2 \alpha \cos^2 \beta - \sin^2 \alpha \sin^2 \beta = \cos^2 \alpha + \cos^2 \beta - 1.$$

Hence, since the positive sign is to be attached to the roots, we have, if β be $< \alpha$,

Intensity required

$$\begin{aligned} &= \frac{\pi \rho^2 \sin \beta}{\sin^2 \alpha} \{2 - (\cos \beta - \cos \alpha) - (\cos \beta + \cos \alpha) - \sin^2 \beta \sin^2 \alpha\} \\ &= \pi \rho^2 \sin \beta (1 - \cos \beta) \left\{ \frac{2}{\sin^2 \alpha} - (1 + \cos \beta) \right\} \\ &= 4\pi \rho^2 \sin \beta \sin^2 \frac{\beta}{2} \left\{ \operatorname{cosec}^2 \alpha - \cos^2 \frac{\beta}{2} \right\}. \quad (7) \end{aligned}$$

And if α be $< \beta$,

Intensity

$$= \pi \rho^2 \sin \beta \left\{ \frac{2}{1 + \cos \alpha} - \sin^2 \beta \right\} = \pi \rho^2 \sin \beta \left\{ \sec^2 \frac{\alpha}{2} - \sin^2 \beta \right\}. \quad (8)$$

In the first case the intensity is clearly least when α is $\frac{\pi}{2}$, its value then being

$$\pi \rho^2 \sin \beta (1 - \cos \beta)^2;$$

and in the second case it is least when α is 0, and its value is

$$\pi \rho^2 \sin \beta \cos^2 \beta.$$

This second minimum will be greater than the other if

$$\cos \beta \text{ is } > 1 - \cos \beta,$$

$$\text{i. e. if } \cos \beta \text{ is } > \frac{1}{2},$$

$$\text{i. e. if } \beta \text{ is } < 60^\circ.$$

If, then, a conical pencil whose semi-vertical angle is less than 60° be passing through the spar, the pencil will be most nearly plane-polarized if the axis of the pencil is at right angles to that of the spar.

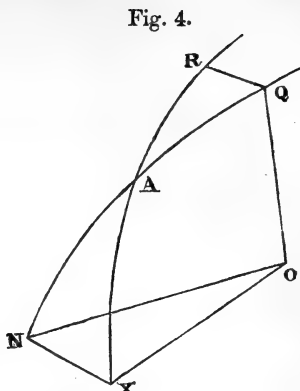
Now if the axis of a conical pencil pass normally through a prism cut as already described, it will be at right angles to the optic axis; and hence the pencil, if its semi-vertical angle be less than 60° , will be more nearly plane-polarized than it would be if the axis occupied any other position. This constitutes a second advantage in favour of the new prism.

Again, suppose we have a parallel pencil of wave-normals in direction ON, and that the axis round which the prism rotates is OX (fig. 4). In our observations we suppose that these two coincide, and work as if the plane of polarization of the emergent light coincided with that of light travelling along OX, thus introducing an error. The amount of this error will depend of course partly on the angle NX (β say), and partly on the angle NXA (ϕ say), OA being the optic axis. If we know β and ϕ we can calculate the error, and could determine the value to be given to XA or α to make it the least possible.

But in practice ϕ may be anything between 0 and 2π , and β anything between 0 and a not very large angle β_1 ; and the question arises, what value must we assign to α in order that the error produced by any chance values of β and ϕ may most probably be as small as possible? To answer this we require to determine, between these limits for β and ϕ , the

mean of the values, irrespective of sign, of the amplitudes of the vibrations normal to the plane OAX.

Let OQ be the direction of vibration for the wave-normal ON, and QR perpendicular on the plane AX. Then the displacement normal to the plane AOX is $\rho \sin QR$; and, with the same notation as before, the average displacement



$$= \frac{\rho}{2\pi} \int \sin \beta \sin \phi \cot \theta d\phi. \quad (9)$$

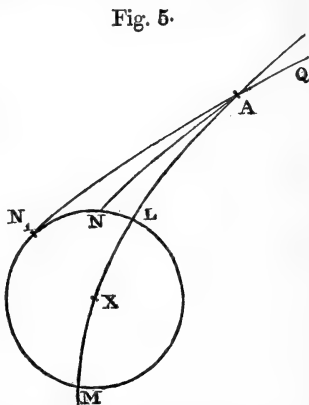
Also $\sin \theta d\theta = \sin \alpha \sin \beta \sin \phi d\phi$.

Hence average displacement

$$= \frac{\rho}{2\pi \sin \alpha} \int \cos \theta d\theta \quad . \quad . \quad . \quad . \quad (10)$$

We must consider now the limits between which the integral is to be taken.

Describe a circle (fig. 5) passing through N with X as centre, and cutting AX in L and M , and if possible let N_1 be a point on this circle such that



$$N_1 A = \frac{\pi}{2}.$$

Suppose we treat displacements to the right of AX as positive. When N coincides with L,

$$\theta = \alpha - \beta.$$

If N lies between L and N₁, Q is to the right of AX, as in figure, and the displacement is positive. We must therefore integrate for θ from $\alpha - \beta$ to $\frac{\pi}{2}$. But if N lie between N₁ and M, Q will be to the left of AX, and the displacement will be negative. Thus, to get the whole average displacement, irrespective of sign, we have to subtract the value of the integral from $\frac{\pi}{2}$ to $\alpha + \beta$. If, however, $\alpha + \beta$ is $< \frac{\pi}{2}$, no position such as N₁ can be found, and we have to integrate straight from

$\alpha - \beta$ to $\alpha + \beta$. The same is true for positions of N on the other side of AX.

Hence, in the first case, the average displacement normal to the plane is

$$\begin{aligned} \frac{2\rho}{2\pi \sin \alpha} \left\{ \int_{\alpha-\beta}^{\frac{\pi}{2}} \cos \theta d\theta - \int_{\frac{\pi}{2}}^{\alpha+\beta} \cos \theta d\theta \right\} \\ = \frac{\rho}{\pi \sin \alpha} \{1 - \sin(\alpha - \beta) + 1 - \sin(\alpha + \beta)\} \\ = \frac{2\rho}{\pi} \left\{ \frac{1}{\sin \alpha} - \cos \beta \right\} \\ = \frac{2\rho}{\pi} \{\operatorname{cosec} \alpha - \cos \beta\}. \quad \dots \quad (11) \end{aligned}$$

And in the second case it is

$$\begin{aligned} \frac{2\rho}{2\pi \sin \alpha} \int_{\alpha-\beta}^{\alpha+\beta} \cos \theta d\theta &= \frac{\rho}{\pi \sin \alpha} \{\sin(\alpha + \beta) - \sin(\alpha - \beta)\} \\ &= \frac{2\rho}{\pi} \cot \alpha \sin \beta. \quad \dots \quad (12) \end{aligned}$$

The first is clearly least when α is $\frac{\pi}{2}$; the second decreases as α increases, but has no minimum; for after a time we should reach a point at which $\alpha + \beta$ became equal to $\frac{\pi}{2}$, and then the limits would require changing: for this value, of course, the two integrals are the same.

Thus the average displacement normal to the plane OAX is least when OX is at right angles to the optic axis, and hence the average error in the position of the plane of polarization is least also. The average displacement just calculated is of course that for a given value of β . If we require the average for any value of β between 0 and β_1 , we must multiply our expressions (11) and (12) by $d\beta$, and, integrating from 0 to β_1 , divide the result by β_1 .

To show the difference in this respect between the new prism and Nicol's, let us calculate the displacement normal to the plane AOX in the two cases, supposing the value of β is 5° .

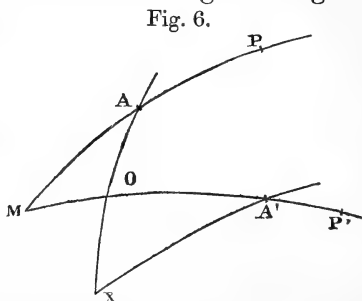
In Nicol's prism, $\alpha = 63^\circ 30'$; so that $\alpha + \beta$ is less than $\frac{\pi}{2}$, and the second formula (12) must be taken. The ratio of the two displacements is therefore

$$\frac{\cot 63^\circ 30' \sin \beta}{1 - \cos \beta} = \cot 63^\circ 30' \cot \frac{\beta}{2};$$

and substituting the value $\beta = 5^\circ$, this comes to 434 : 39, or about 11 to 1. Thus the average error in the position of the plane of polarization as determined by the new prism will be about one eleventh of that which would be produced by the same errors of adjustment with a Nicol's prism; while the amount of light polarized out of the proper plane will be less than one per cent. of that which would be produced by a Nicol.

Again, suppose the prism is turned through an angle ω about OX (fig. 6), and let us inquire what is the angle through which the plane of polarization of the emergent light is rotated. Let OA' be the new position of the optic axis. Join MA, MA', and in them take points P, P' such that

$$MP = MP' = \frac{\pi}{2}. \quad OP, OP'$$



are the directions of vibration for the waves travelling along OM in the two positions of the prism respectively. The angle through which the plane of polarization has been turned is P P' or P M P', that through which the prism has been turned is A X A'; and we require to investigate the conditions under which the average difference between these two for all possible positions of M within a certain distance, β_1 say, of X.

Now we have seen already that if the axis of rotation be at right angles to the optic axis, the average error produced in the determination of the position of the plane of polarization for each of the two positions of the prism will be a minimum; and hence it follows that the average error in the angle between these two positions is a minimum also.

All these results, of course, hold only for the position of the plane of polarization of the light when in the crystal, and will be modified by the refraction that takes place as the waves emerge into the air. But since the ends of the prism are normal to its length, for all the waves considered the incidence is very nearly direct, and the change produced by refraction in the position of the plane of polarization is very small indeed.

Thus a prism cut as described possesses the following advantages over Nicol's prism:—

1. There is no lateral displacement in the apparent position of an object viewed through it.
2. A conical pencil whose axis passes directly through is more nearly plane-polarized than would be the case if the axis of the prism were related to that of the spar in any other manner.

3. If the direction of the wave-normal within the prism does not quite coincide with the axis of rotation, the average error in the position of the plane of polarization is less than for any other method of cutting.

I hope shortly to have some prisms cut by Mr. Hilger in this manner, and to test by means of them the theoretical conclusions arrived at in the paper.

Note added April 26th.

If the plane of section PQ be inclined to BC at an angle of 20° , as in fig. 2, the angular aperture of the field will be small, only about 10° , and it will be necessary that all the light traversing the prism should be very nearly parallel to BC. The aperture may be increased up to about 20° by lengthening the prism considerably and decreasing the angle between PQ and BC. If this be reduced to 11° , the aperture will have its maximum value of 22° .

The aperture may be somewhat increased, and the length of the prism shortened, by using as the separating medium balsam of copaiba, as was suggested at the meeting of the Physical Society at which this paper was read.

The mean index of refraction for this substance is about 1.52, as determined by Brewster. The angle of total reflexion therefore for the ordinary ray is $\sin^{-1}(1.52/1.66)$, or about 66° , while for Canada balsam this angle is about 68° . The possible aperture, using the balsam of copaiba, thus is about 24° .

Professor Thomson's prism, mentioned already, will have a wider field. But it must be remembered that the new prism was not designed for microscopic work, but to obviate the displacement in the image referred to at the commencement of the paper, and to produce a field in which the plane-polarization should be as nearly as possible complete.

LII. *Notices respecting New Books.*

Physical Optics (Text-books of Science). By R. T. GLAZEBROOK, M.A. &c. (London: Longmans, Green & Co. Pp. 434 + xiv.)

THIS is an important elementary work, chiefly on "physical optics;" but it contains also much of what is commonly termed "geometrical optics." It is on the whole most lucidly written, and gives a capital idea of the subject to those who wish to grasp a sound knowledge of it without going into the higher mathematical analysis. The work purports to be adapted for "artisans and students in public and science schools:" it is, however, partly from the general difficulty of parts of the subject and partly from the

mathematical knowledge required, really only adapted for quite advanced classes. The explanation of the rectilinear propagation and of the reflexion and refraction of light as a consequence of the undulatory theory is remarkably clear; and the difficult subjects of diffraction and double refraction are well put. A discussion of Fresnel's and McCullagh's theories of polarized light is given: the discussion is difficult, and the results doubtful and discrepant, so that it seems rather beyond the scope of a "Text-book." The first chapter treats of the wave-motion of light as a form of energy; and the fifteenth chapter discusses the probable relations of light, electricity, and magnetism as vibrations of the same material ether: these chapters are most important in a theoretical view.

The work is well got up, and contains few misprints for the amount of matter. The expression "wave-surface centre A" occurs frequently as short for "wave-surface whose centre is A;" a preliminary explanation would have been useful. The innovation in fractional notation $\sin \phi / \sin \phi'$ is occasionally used (without explanation); having been thus to some extent adopted, it might have been used much more frequently with considerable gain in conciseness.

ALLAN CUNNINGHAM, *Major R.E.*

Transit Tables for 1883 for Popular Use. By LATIMER CLARK, M.I.C.E. (London, A. J. Frost, 1883: pp. 15+103 of Tables.)

THESE may be looked on as a sort of small Nautical Almanac for amateurs for taking time with a small transit, and are sufficient for the purpose when taken in conjunction with the same author's 'Treatise on the Transit Instrument &c. for the use of Country Gentlemen,' reviewed in No. 88 of this Magazine. As an aid to popularizing the use of the portable transit their publication may be welcomed.

ALLAN CUNNINGHAM, *Major R.E.*

LIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 298.]

March 7, 1883.—J. W. Hulke, Esq., F.R.S., President,
in the Chair.

THE following communications were read:—

1. "On Gray and Milne's Seismographic Apparatus." By Thomas Gray, Esq., B.Sc., F.R.S.E. Communicated by the President.

This apparatus was stated to have for its object the registration of the time of occurrence, the duration, and the nature, magnitude, and period of the motions of the earth during an earthquake. The instrument was made by Mr. James White, Glasgow, and is to be used by Prof. John Milne in his investigations in Japan.

In this apparatus two mutually rectangular components of the horizontal motion of the earth are recorded on a sheet of smoked paper wound round a drum, kept continuously in motion by clock-

work, by means of two conical pendulum-seismographs. The vertical motion is recorded on the same sheet of paper by means of a compensated-spring seismograph. In details these instruments differ considerably from those described in the *Philosophical Magazine* for September 1881; but the principle is the same.

The time of occurrence of an earthquake is determined by causing the circuit of two electromagnets to be closed by the shaking. One of these magnets relieves a mechanism, forming part of a time-keeper, which causes the dial of the timepiece to come suddenly forward on the hands and then move back to its original position. The hands are provided with ink-pads, which mark their positions on the dial, thus indicating the hour, minute, and second when the circuit was closed. The second electromagnet causes a pointer to make a mark on the paper receiving the record of the motion. This mark indicates the part of the earthquake at which the circuit was closed.

The duration of the earthquake is estimated from the length of the record on the smoked paper and the rate of motion of the drum. The nature and period of the different movements are obtained from the curves drawn on the paper.

2. "Notes on some Fossils, chiefly Mollusca, from the Inferior Oolite." By the Rev. G. F. Whidborne, M.A., F.G.S.

3. "On some Fossil Sponges from the Inferior Oolite." By Prof. W. J. Sollas, M.A., F.G.S.

4. "On the Dinosaurs from the Maastricht Beds." By Prof. H. G. Seeley, F.R.S., F.G.S.

March 21.—J. W. Hulke, Esq., F.R.S., President,
in the Chair.

The following communication was read:—

"On the supposed Pre-Cambrian Rocks of St. David's." By Archibald Geikie, Esq., LL.D., F.R.S. (Part I.)

The author began by briefly narrating the circumstances under which he had been led to study the geology of St. David's. He had visited the district twice, first in company with Mr. B. N. Peach, with whose cooperation nearly all the field-work was done, and again in conjunction with Mr. W. Topley. The paper was divided into two parts, the first being mainly controversial, and the second descriptive. Only the first part was read.

PART I.

According to Dr. Hicks there are at St. David's three distinct Pre-Cambrian formations:—the "Dimetian," consisting of crystalline, gneissic, and granitoid rocks; the "Arvonian," formed of felsites, quartz-porphyrries, hälleflintas, and other highly silicated rocks; and the "Pebidian," composed of tuffs, volcanic breccias, and basic lavas. He regards the "Arvonian" as later than, and unconformable to the "Dimetian," and the Pebidian as younger than, and

unconformable to both; and he asserts that the basement conglomerate of the Cambrian system lies quite unconformably on all these rocks, and is in great part made up out of their waste.

Taking up each of these groups in the order of sequence assigned to them, the author maintained that the "Dimetian group" is an eruptive granite, which has disrupted and altered the Cambrian strata, even above the horizon of the supposed basal conglomerate. He described a series of natural sections where this relation is exposed, particularly one on the coast at Ogof-Llesugn, where the conglomerate has been torn off and involved in the granite, and has been intensely indurated, so as to become a kind of pebbly quartzite. No other rock occurs within the granite mass except dykes of diabase, which rise through all the rocks of the district, but are especially abundant in the granite. The veins of finer granite, so general in granite areas, are conspicuous here. In short, whether studied in hand-specimens or on the ground, the rock is so unmistakably an eruptive mass that the author could not understand how this view, which was that expressed on the Geological Survey maps, should ever have been called in question. The manner in which it has risen across the bedding of successive horizons in the Cambrian series proves that, instead of being a Pre-Cambrian gneiss, it must be much younger than all the Cambrian rocks of the district.

The "Arvonian group" consists of quartziferous porphyries, or elvans, associated with the granite, and of the metamorphosed strata in their vicinity. Reference was made to natural sections where the actual intrusion of the elvans across the bedding of the rocks could be seen.

The "Pebidian group" comprises a series of volcanic tuffs and breccias, with interstratified and intrusive lavas. The author maintained that this group forms an integral part of the Cambrian system as developed at St. David's. It has been broken through by the granite and porphyries, and is therefore of older date. Instead of being covered unconformably by the Cambrian conglomerate, as asserted by Dr. Hicks, the volcanic group is covered quite conformably by that rock; and seams of tuff are interstratified with the conglomerate and occur on various horizons above it. The conglomerate, instead of being mainly composed of fragments of the rocks beneath it, consists almost entirely of quartz and quartzite, only four per cent. of fragments having been found to have been derived from some of the projecting lava-islands underneath it.

From the evidence now brought forward, the author contended that as the names "Dimetian," "Arvonian," and "Pebidian" had been founded on error of observation, they ought to be dropped out of geological literature.

April 11.—J. W. Hulke, Esq., F.R.S., President, in the Chair.

The following communication was read:—

"On the supposed Pre-Cambrian Rocks of St. David's." By Archibald Geikie, Esq., LL.D., F.R.S., F.G.S.

PART II.

In this second part of his paper the author gave the results of the *Phil. Mag.* S. 5. Vol. 15. No. 95. May 1883. 2 D

survey which he had made of the district with Messrs. Peach and Topley, and of his study of a series of more than 100 thin slices of the rocks collected at St. David's. He found that he could corroborate generally the descriptions of previous writers on the microscopic structure of the rocks, and that investigation with the microscope amply confirmed the deductions he had drawn from observations in the field.

1. *Order of Succession of the Rocks.*—The following rock-groups in the Lower Cambrian series are recognizable at St. David's, and are given in descending order:—

4. Purple and greenish grits, sandstones, and shales.
3. Green and red shales and sandstones, with true tuffs (*Lingulella primæva*).
2. Quartz conglomerate.
1. Volcanic group (tuffs, schists, lavas).

The volcanic group forms the oldest part of the Cambrian series at this locality. The bottom is not reached; but about 1800 feet are visible. It consists mainly of purplish-red, green, grey, and pale tuffs, with occasional breccias and bands of olivine-diabase. Analyses of some of these rocks had been made for the author by M. Renard, of Brussels, and Mr. J. S. Grant Wilson, of the Geological Survey of Scotland. The tuffs are partly basic, derived from the disruption of diabase lavas (48 per cent. of silica), partly acid, from the destruction of fine felsites (72–80 per cent. of silica). The microscopic structure of the tuffs was described, and slides and drawings were exhibited. The lavas are varieties of olivine-diabase. Their augite is remarkably abundant and fresh; and they contain scattered larger, well-formed, as well as imperfect crystals of olivine, generally in the form of hæmatitic pseudomorphs. No instance was observed of a siliceous lava having been erupted at the surface. The felsitic fragments in the tuffs must have been derived from the explosion of lavas that do not seem to have flowed out above ground. It was pointed out that this fact is exactly paralleled in the case of the volcanic group of the Lower Old Red Sandstone in the Pentland Hills.

In relation to the quartz-conglomerate, allusion was made to the constant recurrence of such conglomerates in the series of geological formations, and to the fact that they do not necessarily mark unconformability or the natural base of groups of sedimentary rocks.

2. *Geological Structure of the District.*—It was shown that the rocks have been folded into an isocline or inverted anticline, so that in one half of the plication the dip of the strata is reversed.

The groups above mentioned are found in their proper order on both sides of the axis which runs through the volcanic group. The granite has risen irregularly through the eastern limb of the isocline. Small faults may occur here and there along the edge of the granite, but they do not in any way affect the general structure.

3. *The Foliation of the District.*—There has been extensively developed at St. David's a fine foliation of particular kinds of rock,

more especially of certain fine tuffs and shales, which have passed into the condition of fine silky unctuous hydro-mica-schists or sericite-schists. A series of microscopic slices was described which showed that the original elastic structure of the beds remains quite distinct, though an abundant development of fine flakes of a hydrous mica has taken place. This structure more particularly characterizes the fine parts of the volcanic group; but it occurs also on various horizons in the groups above the conglomerate, thus linking the whole as one great continuous series of deposits. The author connected it with the plication of the district, and pointed out the great interest attaching to these fine schistose bands as revealing some of the incipient stages of the same process that had changed wide regions of sedimentary strata into crystalline schists.

4. *The Granite, Quartz-Porphyrries, and accompanying Metamorphism.*—The petrographical characters of these eruptive rocks were described, and their perfect analogy to the familiar granites and elvans of other districts was pointed out. Specimens were shown illustrating the gradation from a true granite into spherulitic quartz-porphyry. The quartz-porphyrries of St. David's (described by Mr. Davies, Dr. Hicks, and others) exhibit spherulitic structure in an exceptionally perfect manner. Between the felsic-spherulites the base is thoroughly micro-crystalline, and not felsitic. The rocks belong to a group intermediate between granites and felsites. They occur in bosses, elvans, or dykes round the granite, cutting through all horizons of the volcanic group, and approaching, if they do not actually intersect, the quartz-conglomerate. The metamorphism associated with the granites and porphyries is best seen near the latter. It consists chiefly in the intense induration of certain bands of rock which have been converted into flinty aggregates (adinole). The alteration takes place usually along the bedding, which is nearly vertical; but veins of the same siliceous material ramify across the stratification of the shales. Examined microscopically, the adinole is found to have acquired a microcrystalline structure, nests of quartz and orthoclase and porphyritic crystals and plagioclase having been developed, together with fine veins and filaments of crystalline quartz. These veins are here and there crowded with approximately parallel partitions of liquid inclusions showing freely moving bubbles. An analysis of a portion of the adinole, made for the author by M. Renard, shows the percentage of silica to be 78.62 with 5.80 of soda, indicating possibly the formation of albite. The author deferred generalizing on the question of the metamorphism he described, but pointed out that a further study of the St.-David's rocks could hardly fail to throw important light on the theory of metamorphism.

5. *The Diabase Dykes and Sheets.*—These are the latest rocks at St. David's, as they traverse all the others. Their macroscopic and microscopic characters were described; and allusion was made to the perfect fluxion-structure found in many of the dykes.

The paper closed with a summary of the geological history of St. David's. The earliest records are those of the Volcanic group, which

show the existence of volcanic vents in that region in an early part of the Lower Cambrian period. The volcanic accumulations were covered conformably by the Conglomerate and succeeding Cambrian groups; but the same kind of tuffs continued to be ejected after the deposition of the Conglomerate. At a later time this thick conformable succession of beds was plicated, and underwent a partial metamorphism, whereby some of the fine tuffs and shales were converted into sericite-schists. Subsequently a mass of granite rose through one side of the fold, accompanied by elvans of spherulitic quartz-porphry, whereby a second, different and feebler kind of metamorphism was induced. The last episode was that of the diabase dykes, which, crowded together in the granite, suggest that the granite boss stands on an old line of weakness and of escape for eruptive material from the interior.

LIV. *Intelligence and Miscellaneous Articles.*

ON THE USE OF THE TERM "FORCE."

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE anomalies and confusions which Mr. Close points out in the use of the term "Force" (Phil. Mag. April, p. 248) appear to me mainly to arise from the very unfortunate definition which he, in common with other modern physicists, insists on applying to the word. Force proper, he says, is "the rate of change of momentum;" and this he would substitute for the old definition, common to all text-books on Mechanics, that a force is a cause of motion. Now if there is one thing which seems clear, in the framing of terminology, it is that where a well-known word receives a scientific definition, that definition should be in as close accordance as possible with its ordinary use. In the light of this rule, the new definition of force is perhaps one of the worst ever constructed. For, on that definition, force belongs to the science of Dynamics alone. There is no force acting between a weight and the table it lies upon; no force acting between the rim of a fly-wheel and its boss; no force acting between a locomotive and a train, if the speed is uniform; and so on. It is clear that we must either give up the new definition, or re-construct all our old ideas on the subject; with a certainty of importing that confusion to which Mr. Close's letter bears such striking witness*.

Following the old definition, the mode of generation of the new is obvious enough. The effect of a force, where it acts freely, is to increase momentum; and as causes are measured by their effects, it follows that we may measure forces by calculating the rate of

* Mr. Close contributes something of his own to this confusion when, in p. 251, he speaks of F as measured in pounds. How a rate of increase of momentum can be expressed in pounds it does not seem easy to discover.

increase of momentum in a given time : in other words, forces will be proportional to $m \frac{dv}{dt}$. From this it is but a step (though it may be a false step) to say that the force is the rate of increase of momentum, and nothing else, and respectfully to demand that we shall reconstruct the whole of Mechanics in accordance with that definition. Mr. Close takes an illustration from the rate of mortality ; and I will follow him. Disease causes death, and the strength of different diseases, or of the same disease at different times and places, may fairly be measured by the rate of mortality (say the number of deaths per 1000 per diem) which they induce. Mr. Close, if consistent with himself, would therefore define disease as being the rate of mortality—that and nothing else ; and would require all our medical literature to be rewritten, because its language does not square with the new definition.

I think it is time that the advocates of the new view should themselves bring out a book, showing that the ordinary laws and propositions of Mechanics can be worked out on their principles in a way intelligible to the ordinary student. Prof. Tait, in a recent communication to the Royal Society of Edinburgh, made a sort of commencement of such a work : I should be glad to hear of any ordinary student who has derived much comfort and clearness of thought from a study of the result. For an attempt of the kind I refer to, but on the opposite lines, I may perhaps be allowed to point to a recent work of my own, entitled ‘The Student’s Mechanics : an Introduction to the Study of Force and Motion.’

May I be allowed to conclude with one word in reply to Mr. von Tunzelmann’s letter on the Conservation of Energy : it is simply to point out that a periodic function is not the same thing as a constant.

WALTER R. BROWNE.

ON A MODIFICATION IN THE PYCNOMETER.

BY EILHARD WIEDEMANN.

Since determinations of the specific gravity of solids for the purpose of discovering relations between it and their chemical constitution are now more and more frequently made, the improvement of the pycnometer communicated in what follows may not be without interest. By means of it results as exact can be obtained with powders as when larger bodies are employed.

As is well known, in the former method the pycnometer is first weighed empty, then filled with water, then with the substance in question, and finally with water and the substance. If the weights in these cases are P , Π , p and π , the specific gravity of the substance is

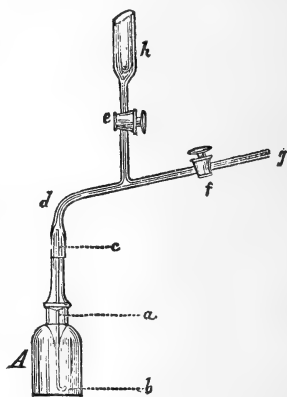
$$s = \frac{p - P}{[\Pi + (p - P)] - \pi}.$$

But air is always contained between the particles of the solid,

and can only be expelled by well boiling, and not always even then. Hence to substances that are decomposed or fused by heating, this method, in the form hitherto used, is not applicable, or at any rate gives extremely inexact results. Also with substances that are easily held in suspension by water or remain on its surface, as finely divided sulphate of barium, the results obtained are inexact, as these substances do not after the boiling sink again to the bottom, and consequently are in part squeezed out with the surplus water on the insertion of the stopper.

For the filling of the pycnometer I have used a method already described by me*, in which the disturbing effect of the air is completely excluded. A is the pycnometer, into which the ground stopper *a* fits; this is passed through by a glass tube, which is bent back at *b* and ends at *c* inside a slip, of which the outer part is attached to the piece *d g* consisting of narrow glass tubes; *f* and *e* are cocks, *h* a small funnel, and *g* the inner part of a slip whose outer part is connected with a good mercury air-pump such as should now be found in every laboratory.

First the pycnometer and the slightly greased stopper are now weighed by themselves; in doing this it is best to suspend the latter on its hook *b* by means of a wire. They are then to be connected with the part *d g*; close *e*, its cavity having been previously filled with a drop of water; open *f*, and exhaust to vacuum; then let the previously well boiled water flow in from *h*;



disconnect the pycnometer at *c*, wipe off the grease from it and weigh it. Then dry it, introduce the powder, and repeat the same procedure. The tube is bent back at *b* in order that the powder may not be too much stirred up when the liquid flows in. If the pycnometer, A, selected is a small one and we first exhaust completely all the space between the cock *f* and the pump, a three or four times repeated action of the pump suffices to make the space in A a vacuum. In the time required for this, even salts containing water, of which the vapour does not possess at all too high a tension, as, for example, the double sulphates of zinc and magnesium with potassium and ammonium, lose only so small a quantity of their water of crystallization that for them sufficiently accurate results are obtained.

As illustrations of the precision of the method, in the following are communicated two series of measurements, made on powdered glass and precipitated sulphate of barium. They were carried out

* Wied. *Ann.* xvii. p. 561 (1882).

with great care by R. Schulze. For each single measurement from about a half to three quarters of an hour was required.

I. Glass Powder.

P	II	p	π	Temp.	s
9.2554	19.4095	14.3788	22.4468	15.7	2.4559
9.2554	19.4095	10.5368	20.1689	16.4	2.4547
9.2554	19.4095	13.608	21.9894	15.1	2.4553
9.2554	19.4095	13.271	21.7895	16.4	2.4551

II. Sulphate of Barium.

P	II	p	π	Temp.	s
9.2554	19.4095	10.5962	20.445	14.9	4.3958
9.2554	19.4095	11.7652	21.3484	14.9	4.3962
9.2554	19.4095	14.333	23.3323	14.5	4.3969
9.2554	19.4095	13.0445	22.3368	14.5	4.3967

The specific gravities differ from one another by not quite a unit in the third decimal place, a result which may be regarded as extremely satisfactory.

If a greater number of determinations of specific gravity have to be effected, it would be advisable to employ several pycnometers, attached to different arms connected with the air-pump and exhausted simultaneously. This would effect a pretty considerable saving of time.—Wiedemann's *Annalen*, 1882, xvii. pp. 983-985.

ON THE UPPER LIMIT OF THE PERCEPTIBILITY OF SOUNDS.
BY E. PAUCHON.

I have the honour of communicating to the Academy the results of some researches on the determination of the upper limit of perceptibility of sounds, a portion of which were made in conjunction with M. L. Bertrand.

I proposed to ascertain if, as several physicists have alleged (although they have not brought forward any experiment proving their assertion), this limit varies, for one and the same ear, with the intensity of the sound. For this purpose I employ a powerful Caignard-Latour siren, modified in some of its parts, and set in action by a jet of steam. Various arrangements, described in a

special memoir, give the possibility of varying the pressure of the steam in the box and the intensity of a sound of a given pitch.

I thus prove that, when the pressure of the steam inside the box varies from 0·5 to 1·5 atmosphere, the limit of perceptibility corresponds to sounds the pitches of which are comprised between 48,000 and 60,000 simple vibrations. When the siren is furnished with a counterplate, the ratio between the pressure of the steam and the velocity of rotation is constantly too high, and, under the conditions of the experiment, the limit of perceptibility can no longer be attained, not even for the sound of 72,000 vibrations, the shrillest which I have been able to produce. Under these conditions the pressure of the steam in the box of the siren reaches 2·5 atmospheres, the velocity of rotation of the disks amounts to 600 turns, and their circumferential velocity to 113 metres, per second. The supply of steam is considerable, about equivalent to that of an engine of 8 horse-powers.

I afterwards caused metal rods fixed at one end to vibrate longitudinally, by rubbing them with cloth sprinkled with rosin. By gradually lessening the length of the vibrating rod the extinction of the sharp sound is easily produced. I remark that

(1) The length of the rod that gives the limiting sound is, for one and the same metal, independent of the diameter.

(2) For steel, copper, and silver, the lengths are to one another in ratios sensibly equal to those of the velocity of propagation of sound in those metals.

Thus, taking the ratio belonging to copper as unit, we get:—

For copper	1·000
For steel	1·002
For silver	0·995

These two observations cannot accord with the results furnished by the siren unless, for these three metals and for the length corresponding to the limiting sound, the lengthening due to the vibration is the same. I have not been able to value with sufficient certainty these minute lengthenings; but, on the other hand, I have ascertained that—

(1) If a hearing-horn be applied to the ear, the limit of perceptibility is slightly extended.

(2) If the rods are excited with different substances—rosin, oil of turpentine, alcohol, ether (which evidently has the effect of modifying the amplitude of the vibration), the limiting length changes, and may vary from a single to a double length: it is the minimum with rosin, which produces the most energetic friction.

(3) The excessively sharp sound that has ceased to be perceptible to the ear, still acts powerfully on the sensitive flame.—*Comptes Rendus de l'Académie des Sciences*, April 9, 1883, t. xcvi. pp. 1041–1043.

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JUNE 1883.

LV. *Colour-Sensation.* By H. R. DROOP, M.A.*

THE generally received theory of colour-sensation is that there are three colour-senses in the eye, and that all the different impressions of colour received by the brain are due to those three colour-senses being affected in different proportions by the light entering the eye. This theory was originally propounded by Young, and was revived by Helmholtz and Maxwell, who established experimentally certain laws of colour-sensation, from which laws the theory of three colour-sensations was a probable, but (as I shall show further on) not a necessary, inference.

Maxwell proved experimentally† that a linear equation of the form

$$X = vV + cC + uU$$

could always be found, expressing any colour and shade of colour perceived by the normally constituted eye in the terms of any three given colours (whether pigments—*e. g.* vermilion, chrome yellow, and ultramarine—or selected rays of the spectrum) as seen by the same eye. This equation, when the coefficients *v*, *c*, and *u* are all positive (*i. e.* when the three given colours are sufficiently intense and distinct from each other), means that the colour *X* could be produced by combining together (*i. e.* presenting to the eye simultaneously) certain proportions of the three given colours. From this it obviously

* Communicated by the Physical Society; read April 28, 1883.

† See Transactions of the Royal Society of Edinburgh, vol. xxi.; Philosophical Transactions, 1860, p. 57.

followed that every such colour and shade *could* be produced by three colour-sensations, each of which, when excited, conveyed to the brain the impression of a homogeneous colour.

Helmholtz arrived at the same conclusion by proving (see *Handbuch der physiologischen Optik*, p. 282, ed. 1867) that any given colour could be produced by combining a certain quantity of white with some particular colour of the spectrum. From which he deduced that every colour and shade of colour depended on only three independent variables, viz. the quantity of the spectrum-colour, the quantity of white, and the length of wave of the spectrum-colour. But though the theory of three colour-sensations was the simplest and most obvious explanation of the experimental facts thus established, it is not (as has been commonly assumed) the only theory capable of explaining them. They would be equally well explained by supposing that there are four, five, or more colour-sensations connected by a sufficient number of linear equations of condition to reduce the number of independent variables to three. Obviously this will satisfy all that Helmholtz established. That it will also explain the law established by Maxwell may be shown as follows.

Suppose that there are four colour-sensations, R, Y, G, and B (the red, yellow, green, and blue sensations), and that each of them is expressed as a linear function of the three standard colours, V, C, and U, as every colour seen by a normal human eye can be expressed.

Then we shall have four equations of the form

$$\left. \begin{aligned} R &= v_r V + c_r C + u_r U, \\ Y &= v_y V + c_y C + u_y U, \\ G &= v_g V + c_g C + u_g U, \\ B &= v_b V + c_b C + u_b U. \end{aligned} \right\} \quad \cdot \cdot \cdot \cdot \cdot (A)$$

And when we eliminate V, C, and U between these four equations we shall get a linear relation between R, Y, G, and B, which is the condition that these four colour-sensations should be capable of being expressed as linear functions of V, C, and U, *i. e.* should be colour-sensations coexisting in a normal eye.

If we supposed five colour-sensations, we should have five equations (A) between which to eliminate V, C, and U, and should get two linear equations between the five colour-sensations.

The proposition that four colour-sensations with a linear relation between them will satisfy Maxwell's law may also be tested in another way, viz. by assuming that there are four colour-sensations connected by a linear equation

$$rR + yY + gG + bB = 0, \quad \cdot \cdot \cdot \cdot \cdot (1)$$

and that V, C, and U, the three standard colours, are known to result from these colour-sensations being effected in certain proportions.

E. g. V is known to result from R, the red colour-sensation, being effected to an extent r_v , Y to an extent y_v , G to an extent g_v , and B to an extent b_v ; and may therefore be represented by the equation

$$V = r_v R + y_v Y + g_v G + b_v B; \quad . \quad . \quad . \quad . \quad (2)$$

and similarly C and U may be represented by

$$C = r_c R + y_c Y + g_c G + b_c B, \quad . \quad . \quad . \quad . \quad (3)$$

$$U = r_u R + y_u Y + g_u G + b_u B. \quad . \quad . \quad . \quad . \quad (4)$$

Then from these four equations we can express R, Y, G, and B as linear functions of V, C, U. But every possible colour X must be produced by exciting all or some of the four colour-sensations, and therefore must be capable of being expressed by an equation

$$X = r_x R + y_x Y + g_x G + b_x B.$$

Consequently, if R, Y, G, and B be replaced in this equation by the linear functions of V, C, and U which represent them, every such colour X can be expressed as a linear function of V, C, and U; *i. e.* every such colour will conform to the law, which Maxwell and Helmholtz established, of being capable of being made up of any three standard colours.

The same reasoning might obviously be applied in like manner to five colour-sensations connected by two linear equations of condition, or to n colour-sensations connected by $n-3$ linear equations of condition.

I have taken up this question and endeavoured to show that what Maxwell and Helmholtz established is not inconsistent with the existence of four or more colour-sensations (provided certain relations exist between them), because a certain recent discovery seems to me to have given a particular hypothesis, involving four colour-sensations, a strong claim to be accepted as, in the main, true. This discovery relates to the colours actually seen by colour-blind persons. Two persons have been discovered who, being each colour-blind of only one eye, can explain how far the colours seen by their colour-blind eyes agree with, or differ from, those seen by their normal eyes. It has been found that each of these persons has two colour-sensations complementary to each other. One sees yellow and blue, and is blind to red and green; while the other sees red and bluish green, and is blind to blue and yellow; and with each of them the combination of his two colour-sensations in proper proportions produces white or grey. Professor Holmgren, of Upsala, has given an account of both these cases in the 'Proceedings of the Royal Society,' vol. xxxi. p. 302; and Pro-

fessor Hippel, of Giessen, has given an account (differing in some respects) of the first or blue-yellow case in Gräf's *Archiv für Ophthalmologie*, vol. xxvi. p. 176, vol. xxvii. pt. 3, p. 47.

These two cases suggested to Professor Preyer of Jena a theory (which he propounded in 1881 in Pflüger's *Archiv*, vol. xxv.) that ordinary eyes have two pairs of colour-sensations—(1) yellow and blue, and (2) red and bluish green, and that the colour-blindness which consists in confusing red and green, or, as the case may be, blue and yellow, is due to the absence of one pair of these sensations. But Professor Preyer does not deal with the difficulty that Helmholtz and Maxwell are supposed to have proved, that there cannot be more than three colour-sensations, although that view is treated as unquestionable by other recent writers, *e. g.* by Professor Donders (Gräf's *Archiv für Ophthalmologie*, vol. xxvii.), and by Professor v. Kries, of Freiburg im Breisgau (*Die Gesichtsempfindungen und ihre Analyse*, Leipzig 1882, p. 33); and it is naturally a serious obstacle to the fair consideration of Professor Preyer's theory.

But inasmuch as Professor Preyer supposes that in each pair of his colour-sensations the one sensation is complementary to the other, we have the equation

$$R + G = \text{White} = Y + B,$$

a linear relation between the four colour-sensations; and therefore it follows, from what I have already proved, that this hypothesis of two pairs of complementary colour-sensations is quite consistent with what Maxwell and Helmholtz established.

This theory of Professor Preyer's explains the leading facts of colour-blindness, viz. that a colour-blind eye only perceives two homogeneous colours, and that it is unable to distinguish between red and green, or, as the case may be, between blue and yellow. It is impossible to ascertain with absolute certainty that persons who are colour-blind with both eyes see the same colours as the two persons who have been discovered colour-blind of only one eye; but it is noteworthy that when Dr. Pole made that minute examination of his colour-blindness, the results of which he gave in the 'Philosophical Transactions' for 1859, he came to the conclusion that the colours he saw were yellow and blue and, as the result of their mixture, white; and he only gave up this view in deference to the three-sensation theory then supposed to be conclusively established.

The following facts, not connected with colour-blindness, seem to me to give considerable support to the hypothesis of two pairs of complementary colour-sensations:—

(1) Observations have been made as to the sensibility of

different parts of the retina to different colours, and also as to the effect of diminishing the angles subtended at the eye by small coloured objects; and in both cases red and green colours are found to comport themselves alike, and differently from blue and yellow. When an object is viewed more and more indirectly, so that its image moves from the yellow spot towards the circumference of the retina, sensibility to yellow and blue lasts longer than sensibility to red and green; while, on the other hand, if the angular magnitude of the object be diminished, sensibility to red and green lasts longer than sensibility to blue and yellow (von Kries, *Gesichtsempfindungen*, pp. 93, 95).

(2) In cases where the colour-senses become affected by disease of the eye, the order in which different colours are found to disappear agrees with the theory of four colour-sensations. In cases of atrophy of the optic nerve, it seems pretty clearly established that green becomes invisible first, then red, then yellow, while the perception of blue remains the longest (see Leber, *Archiv für Ophthalmologie*, vol. xv.; Leber, *Handbuch der Augenheilkunde*, vol. v. p. 1039; Schön, *Lehre vom Gesichtsfelde und seine Anomalien*).

In cases where the sight is affected by excess in alcohol or tobacco, Nuel found that green and red became invisible simultaneously, and blue and yellow later (*Annales de l'Oculiste*, 80, p. 110, as cited in von Kries, *Gesichtsempfindungen*, p. 156).

On the other hand, the received hypothesis of three colour-sensations does not readily explain how it is that one colour-blind eye sees blue, yellow, and white, and another red, green, and white, as Professors Holmgren and Hippel have found to be the case. If there are only three colour-sensations, it is generally agreed that they must be red, green, and violet. If one of these three sensations were wanting, we should naturally expect that the defective eye would have the other two sensations of a normal eye. For instance, if the red sensation were missing, we should expect that the eye would see green and violet, and white would appear of a bluish green complementary to the missing red. Similarly if green were missing, we should expect that the defective eye would see red and violet; while if violet were missing, it would see red and green, and white would appear yellowish complementary to violet.

The only attempted explanation* I have seen of this pro-

* Professor Holmgren, when communicating the two cases of one-sided colour-blindness to the Royal Society, states that he considers these phenomena quite consistent with the theory of three colour-sensations; but he does not explain how he reconciles them with it, except by referring to works which I have not been able to get access to.

ceeds from Professor Donders, who suggests that where one of the three colour-sensations has been wanting from birth, its absence may have modified the development of the other two sensations. He says:—"The retina is not an instrument with three strings of which one has suddenly snapped. It is a living instrument whose three differently toned strings have been developed in conjunction with each other" (Gräff's *Archiv für Ophthalmologie*, vol. xxvii. p. 212).

This explanation is ingenious; but it assumes that all cases of colour-blindness to red and green are from birth, whereas, though this is usually so, there are several alleged cases of acquired colour-blindness to red and green. Dr. Joy Jeffries, pp. 50-52, gives two cases, one discovered by Professor Tyndall, the other by Mr. Haynes Walton; and M. Nuel has also described one, *Annales de l'Oculiste*, 80, 82, as quoted by v. Kries, *Gesichtsempfindungen*, p. 154.

Moreover Professor Cohn (*Deutsche medicinische Wochenschrift*, 1880, No. 16, cited by von Kries, p. 158) claims to have temporarily restored to normal colour-vision a person affected with red-green colour-blindness from birth. If this be so, there cannot well have been any such abnormal development of his other colour-sensations as Professor Donders supposes. Such a development could hardly have been suddenly cured by artificial means.

Those colour-blind persons who cannot distinguish between red and green have been divided into two classes, according as they can perceive rays towards the end of the spectrum or are unable to do so. According to many adherents of the theory of three colour-sensations, those who can perceive rays at the red end want the green-colour sense, while those who cannot do so want the red sense. It seems to be established that rays at the green part of the spectrum do not make as much impression on the so-called green-blind as on the so-called red-blind; and Professor Donders has lately ascertained by careful measurements with two cases, one of so-called red-blindness, and the other of so-called green-blindness, that throughout the red and orange parts of the spectrum the red-blind eye perceives less light than the green-blind eye, and that the opposite is the case in the green portion of the spectrum (Gräff's *Archiv für Ophthalmologie*, vol. xxvii.; Transactions of the International Medical Congress for 1881, vol. i. p. 277).

But there seem to me to be several serious objections to explaining the difference between so-called green-blind and so-called red-blind by the hypothesis of three colour-sensations:—

(A) If there are only three colour-senses, the green sense

must be of a yellowish green capable of producing yellow and orange when combined with the red sense, and very different from the bluish green which is complementary to red. Therefore if one class of colour-blind persons have lost the red sense and the other the green sense, there ought to be considerable differences in all the colour equations obtained from the two classes of eyes, and especially in the proportions of blue and yellow which will neutralize each other. But I have not met with any trace of such differences having attracted attention, except in equations between red and green, and it is clear that in the spectrum the neutral point where the blue or violet colour-sense neutralizes the other colour-sense is very nearly the same for red-blind and green-blind persons. Professor Donders fixes it for his red-blind case at a wave-length of 494·85 millionths of a millimetre, and for his green-blind case at 502·3 millionths, the difference 7·5 being not one fiftieth part of the difference between the greatest and least wave-lengths in the visible spectrum; while Professor Preyer, in another case, found that doubling the amount of light altered the neutral point from 512·8 to 506·6, *i. e.* nearly as much (Pfüger's *Archiv*, vol. xxv.).

(B) If blindness to the red end of the spectrum were due to the absence of the red sense, it would be the same in extent in different red-blind persons, whereas in fact it differs considerably. (Donders, Gräfs *Archiv*, vol. xxvii.)

(C) The extent to which the violet end of the spectrum is obscured to violet- or blue-blind eyes also varies very much. Professor Stilling (*Klinische Monatsblätter für Augenheilkunde*, 1875, Beilage 2) met with three cases in which the green thallium-line between E and D formed the boundary of the visible spectrum; while in another case (*Centralblatt für praktische Augenheilkunde*, 1878, p. 99) the same observer found that nearly the whole of the spectral green was perceived, and grey or, in a faint spectrum, red beyond it. In the case of one-eyed violet- or blue-blindness described by Professor Holmgren ('Proceedings of the Royal Society,' vol. xxxii. p. 305) "the spectrum is continued over the place where we see green, greenish blue, cyan-blue, and indigo to the commencement of the violet, where it absolutely ended with a sharp limit about Fraunhofer's line G."

(D) As is well known, inability to distinguish between red and green has in many cases been found to exist among different members of the same family, and especially among brothers; and therefore when such colour-blindness is found to exist among relations, there is a very strong probability that they have inherited the same affection of the eyes. There-

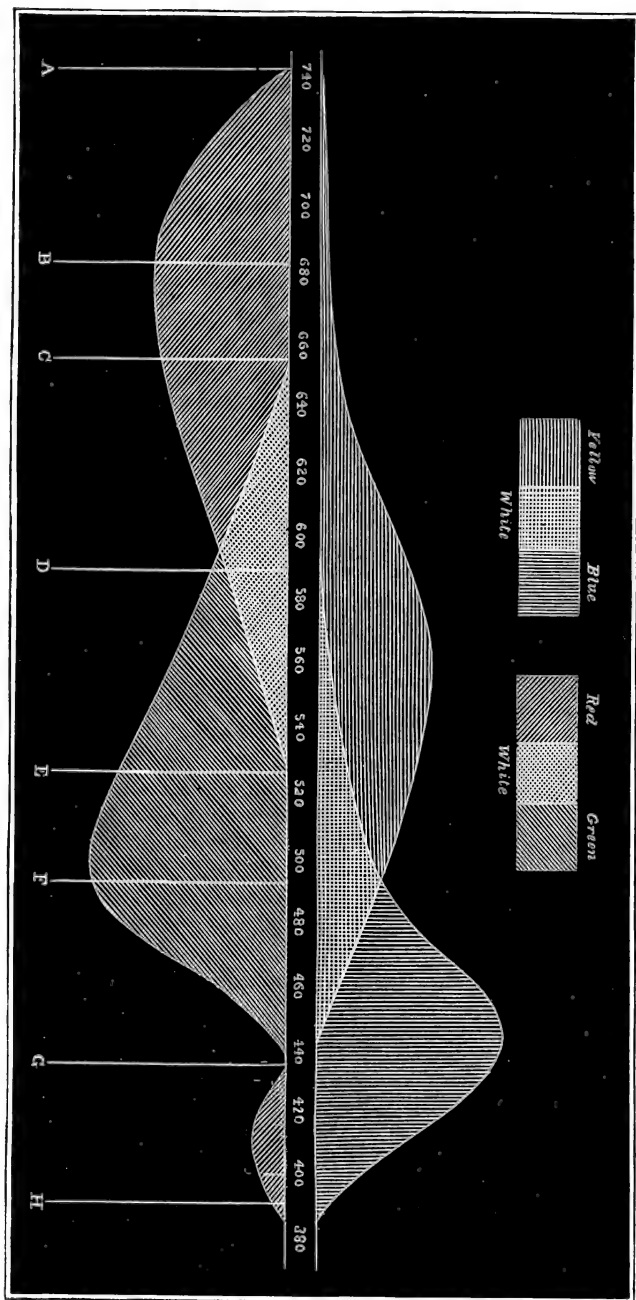
fore if red-blindness and green-blindness be distinct things, due to the absence of different colour-senses, we should expect that the colour-blind members of the same family would be either all red-blind or all green-blind. But this is not the case. Among the colour-blind cases examined by Professor Stilling (*Klinische Monatsblätter für Augenheilkunde*, 1875, Beilage 1 and 2) there were two pairs of brothers both unable to distinguish between red and green; and in each case one brother was able to perceive light at the red end of the spectrum, while the other was not.

All these reasons lead me to believe that the difference between so-called red-blind and so-called green-blind is not due to their having lost different colour-senses, but rather to the loss of one pair of colour-senses, those for red and green being, in the case of the so-called red-blind, accompanied by some disturbance of the other pair of colour-senses—a disturbance varying in character and degree in different cases, and similar to what is found to exist in different cases of blue- or violet-blindness.

The shapes of the curves Professor Donders has published (Trans. International Medical Congress, 1881, vol. i. p. 280) to represent the respective intensities of the light perceived by a red-blind and a green-blind eye have suggested to me a possible explanation of the difference between these two eyes. The curves representing the less-refrangible sensation of each eye correspond very nearly in shape and dimensions; but that for the red-blind eye is shifted some way further from the red end of the spectrum. On the other hand, the curves representing the more-refrangible sensation of the two eyes are almost identical in position as well as in shape and size. This effect would be produced if the organization producing the less-refrangible (or yellow) colour-sensation in the green-blind eye were so modified in the red-blind eye as to make shorter waves produce the same effects which longer waves produced in a green-blind eye. The change supposed would be equivalent to shifting the tone of a musical instrument an octave higher.

But I do not suppose that all cases of shortened spectrum could be thus accounted for.

In order to show more in detail what my views of colour-sensation are, I have prepared a diagram representing roughly how the two pairs of colour-senses are affected by the different rays of the spectrum. The upper half of the diagram represents the effects produced by the different rays on the yellow and blue colour-senses, and the lower part those they produce on the red and green colour-senses.



The portion with crossed vertical and horizontal lines represents the extent to which the same rays operate on both the yellow and the blue colour-senses, and thus produce white light, which (combining with the colour produced by the colour-sense which is most affected) produces a whitish yellow or a whitish blue. Similarly the portion crossed by diagonal lines in the lower half of the diagram represents the extent to which the same rays operate on both the red and the green colour-senses.

I have represented the complementary colour-sensations as thus overlapping*, because Professors Preyer and Hippel have found that, for red-green blind eyes, the neutral point in the spectrum which appears white or grey varies in position according to the intensity of the light (Pflüger's *Archiv*, vol. xxv.; Gräff's *Archiv für Ophthalmologie*, vol. xxvii. pt. 3). Professor Preyer found that enlarging the aperture through which the light was admitted from .250 millim. to .370 millim. shifted the neutral point, where the spectrum appeared grey, from where the wave-length was 512.8 millionths of a millimetre to where it was 506.6 millionths. This is readily intelligible, if the rays in this part of the spectrum affect both the yellow and the blue colour-senses, while the intensity of the light alters the proportions in which they are respectively affected by it.

It will be observed that a narrow strip of yellow extends nearly to the red end of the spectrum. Professor Holmgren ('Proceedings of the Royal Society') states that the red seen by the violet-blind eye, in his case of one-sided violet-blindness, is not quite identical with the common spectral red of the normal eye, but rather a clearer red having a shade of carmine, about the same as the red towards the end of the subjective spectrum of the normal-eyed. This colour would obviously require a slight admixture of yellow to reduce it to the common spectral red of the normal-eyed. Moreover the extension of the yellow nearly to the red end of the spectrum explains how it is that a great many persons who are red-green colour-blind can see almost to the red end of the spectrum.

The strip of red at the violet end of the spectrum also requires explanation. The colours seen by the red-green blind eye in the case of one-sided red-green blindness are not yellow and violet, but yellow and indigo with only a faint shade of

* The extent to which the colour-sensations are represented as overlapping rests on conjecture. Observations on colour-blind persons, *i. e.* persons with only two colour-sensations, give readily the neutral points where the sensations counterbalance each other and produce white; but the extent to which they overlap could only be inferred from observing when the colours cease to have any admixture of white.

violet in it. Indeed, while Professor Holmgren speaks to the faint shade of violet ('Proceedings of the Royal Society'), Professor Hippel, who discovered this case and had more opportunities of examining it, states that the blue lines of indium and cæsium (which are indigo, not violet) appeared the same to both eyes (Gräff's *Archiv*, vol. xxvii.). Therefore some addition is necessary to produce the deeper violet tints of the spectrum; and this can only be obtained by supposing that the violet rays affect the red colour-sense as well as the blue one.

This hypothesis, that violet results from combining the blue and red colour-sensations, is part of Professor Preyer's theory (see Sect. 38 of his paper in Pflüger's *Archiv*), and seems to me to be supported by several other facts.

(1) As I have already mentioned, when an object is viewed more and more indirectly, so that its image moves from the yellow spot towards the circumference of the retina, sensibility to yellow and blue lasts longer than the sensibility to red and green. On the other hand, if the angular magnitude of a coloured object be diminished, sensibility to red and green lasts longer than sensibility to blue and yellow. In each case violet behaves like a compound of blue and red. As the image moves towards the circumference of the retina, the violet object passes through blue into white, the red fading first; while, as the angular magnitude of a violet object is diminished, it becomes reddish (v. Kries, *Gesichtsempfindungen*, p. 93).

(2) Again, Nuel (*Annales de l'Oculiste*, 80, 82, cited by v. Kries, p. 154) describes how in a certain case of acquired colour-blindness "violet appears blue, red and green white." Similarly Schön states that in cases of atrophy of the optic nerve, when green, red, and yellow are no longer recognized, blue alone is correctly designated, and violet is distinguished as dark blue (*Lehre vom Gesichtsfelde*, p. 23, cited by von Kries, p. 155).

(3) I have already mentioned a case of yellow-blue blindness described by Stilling, in which blue and violet were, in a faint spectrum, designated as red, though in a brighter spectrum they seem to have appeared grey.

All these facts seem to me to point to violet being the result of affecting at once the blue and red colour-senses.

I am moreover disposed to think that, in addition to the two pairs of complementary colour-senses, there is a fifth colour-sense for white.

Inasmuch as

$$R + G = Y + B = \text{White},$$

we have the two linear equations between five colour-sensations which are required to satisfy the laws which Maxwell and Helmholtz established. Therefore the hypothesis of a fifth, white, colour-sense is admissible.

That the eye does perceive white separately from any other colour is rendered at least probable by considering some particular cases in which this seems to occur.

(1) When an object is viewed indirectly, so that the image falls upon a part of the retina at a sufficient distance from the yellow spot, it will appear white or grey, whatever its actual colour may be (von Kries, pp. 91-95).

(2) If the angular dimensions of a coloured object be diminished, it will ultimately appear white or grey (von Kries, pp. 87, 94).

The more probable explanation in both these cases seems to be that the other colour-senses are no longer affected by the object, and only the white colour-sense remains affected by it.

(3) Every colour when intensely lighted up appears white (von Kries, p. 81). A not improbable explanation of this seems to be that the other colour-senses are only capable of being affected by light to a limited extent as compared with the white colour-sense.

(4) In cases of atrophy of the optic nerve the perceptions of different colours are gradually lost, until at length every colour appears grey (von Kries, p. 154).

(5) There are also cases of total colour-blindness from birth, when every thing appears of the same colour with only different degrees of light and darkness. When this affects both eyes completely, it is of course impossible to predicate with absolute certainty what colour is perceived. But Becker (Gräff's *Archiv*, vol. xxv.) describes a case where only one eye was so affected, the other having normal vision; and I have seen another case described in which one half of each eye was completely colour-blind, the other half being normal. In each of these cases the colour-blind vision was of white. This white vision must have been arrived at either through the other colour-senses having been lost, leaving a white colour-sense behind, or through their having been modified into white.

LVI. *On the Vibrations of a Cylindrical Vessel containing Liquid.* By Lord RAYLEIGH, F.R.S., Professor of Experimental Physics in the University of Cambridge*.

THE problem of a uniform cylinder vibrating in two dimensions is considered in my book on the Theory of Sound, § 233. If the displacements at any point a, θ of the circumference be $\delta r, a\delta\theta$, then for a single component

$$\delta r = aA_n \cos n\theta, \quad \delta\theta = -n^{-1}A_n \sin n\theta. \quad . \quad . \quad (1)$$

If d be the thickness, and σ the volume-density of the material, the kinetic energy of the motion for a length z measured parallel to the axis is

$$T = \frac{1}{2}\pi z d \sigma a^3 (1 + n^{-2}) \left(\frac{dA_n}{dt} \right)^2. \quad . \quad . \quad . \quad (2)$$

The corresponding potential energy is

$$V = \frac{\pi B z}{2a} (n^2 - 1)^2 A_n^2,$$

in which B is a constant depending upon the material and upon the thickness. As a function of thickness $B \propto d^3$; so that we may write $B = B_0 d^3$, in which B_0 depends upon the material only. Thus

$$V = \frac{\pi B_0 d^3 z}{2a} (n^2 - 1)^2 A_n^2. \quad . \quad . \quad . \quad (3)$$

If the cylinder be empty, these expressions suffice to determine the periods of vibration. Thus, if $A_n \propto \cos p_0 t$,

$$p_0^2 = \frac{B_0 d^2}{\sigma a^4} \frac{(n^2 - 1)^2}{1 + n^{-2}}, \quad . \quad . \quad . \quad (4)$$

showing that for a given material the frequency is proportional to the thickness and inversely as the square of the radius.

If the cylinder contain frictionless fluid, the motion of the fluid will depend upon a velocity-potential ϕ which satisfies the equation

$$\left(\frac{d^2}{dr^2} + \frac{1}{r} \frac{d}{dr} + \frac{1}{r^2} \frac{d^2}{d\theta^2} + \frac{d^2}{dz^2} + \kappa^2 \right) \phi = 0, \quad . \quad . \quad . \quad (5)$$

in which

$$\kappa = a'^{-1} p, \quad . \quad . \quad . \quad (6)$$

a' being the velocity of propagation of sound within the fluid. If the fluid can be treated as incompressible, we may put $\kappa = 0$. For the present purpose we will retain κ , but we will assume

* Communicated by the Author.

that the motion is strictly in two dimensions. Introducing the further assumption that $\phi \propto \cos n\theta$, we get in place of (5),

$$\left(\frac{d^2}{dr^2} + \frac{1}{r} \frac{d}{dr} - \frac{n^2}{r^2} + \kappa^2\right)\phi = 0, \quad . \quad . \quad . \quad (7)$$

of which the solution is

$$\phi = a_n \cos pt \cos n\theta J_n(\kappa r). \quad . \quad . \quad . \quad (8)$$

The relation between a_n and A_n of (1) is readily found by equating the value of $d\phi/dr$, when $r=a$, to $d\delta r/dt$, both of which represent the normal velocity at the circumference. We get

$$a_n \cos pt = \frac{a}{\kappa J'_n(\kappa a)} \frac{dA_n}{dt}. \quad . \quad . \quad . \quad (9)$$

The kinetic energy of the fluid motion is given by

$$\begin{aligned} T &= \frac{1}{2}\rho \iiint \left\{ \left(\frac{d\phi}{dx}\right)^2 + \left(\frac{d\phi}{dy}\right)^2 \right\} dx dy dz \\ &= \frac{1}{2}\rho \left[z \int \phi \frac{d\phi}{dr} a d\theta + \kappa^2 \iiint \phi^2 dx dy dz \right] \\ &= \frac{1}{2}\pi \rho z a^2 \cos^2 pt \left\{ \kappa a \cdot J_n(\kappa a) J'_n(\kappa a) + \kappa^2 \int_0^a J_n^2(\kappa r) r dr \right\}. \quad (10) \end{aligned}$$

For the potential energy of the liquid, if compressible, we have

$$\begin{aligned} V &= \frac{1}{2}\rho a'^{-2} \iiint \left(\frac{d\phi}{dt}\right)^2 dx dy dz \\ &= \frac{1}{2}\pi \rho z a_n^2 \sin^2 pt \kappa^2 \int_0^a J_n^2(\kappa r) r dr. \quad . \quad . \quad . \quad (11) \end{aligned}$$

The sum of the potential and kinetic energies for the solid and liquid together must be independent of the time. The unintegrated terms in (10) and (11) cancel, and we find

$$\frac{B_0 d^3 (n^2 - 1)^2}{a^3 p^2} = a \sigma d (1 + n^{-2}) + \frac{\rho a}{\kappa} \frac{J_n(\kappa a)}{J'_n(\kappa a)}. \quad . \quad . \quad (12)$$

In the application of (12) κa is a small quantity. From the ascending series for $J_n(\kappa a)$ we find

$$\frac{a}{\kappa} \frac{J_n(\kappa a)}{J'_n(\kappa a)} = \frac{a^2}{n} \left(1 + \frac{\kappa^2 a^2}{n \cdot 2n + 2} + \dots \right), \quad . \quad . \quad (13)$$

so that approximately

$$\frac{B_0 d^3 (n^2 - 1)^2}{a^4 p^2} = \sigma d (1 + n^{-2}) + n^{-1} \rho a \left(1 + \frac{\kappa^2 a^2}{n \cdot 2n + 2} \right). \quad . \quad (14)$$

If p_0 be the value of p when $\rho=0$,

$$\frac{p_0^2}{p^2} = 1 + \frac{n}{n^2+1} \frac{\rho a}{\sigma d} \left(1 + \frac{\kappa^2 a^2}{n \cdot 2n+2} \right). \quad (15)$$

From (14) or (15) we see that the effect of a finite as compared with an infinitely small compressibility is to *increase* the depression of pitch due to the fluid. As the velocity of sound is greater in liquids than in air, it would seem that $\frac{1}{12} \kappa^2 a^2$ would generally be negligible. In this case, for the principal mode of vibration corresponding to $n=2$, (15) becomes simply

$$\frac{p_0^2}{p^2} = 1 + \frac{2}{5} \frac{\rho a}{\sigma d}. \quad (16)$$

In Auerbach's recent paper upon this subject* various observations upon the depression of pitch due to the action of liquid are given. In his notation $p_0/p = G$. From (15) we see that if G_0 be the value of G for water, the same vessel being used in both cases,

$$\frac{G^2-1}{G_0^2-1} = s, \quad (17)$$

if s denote the specific gravity of the liquid, referred as usual to water as a standard. Auerbach's observations are fairly accordant with (17); and there seems to be scarcely sufficient warrant for attributing the discrepancies to the influence of compressibility.

In observations with different vessels of the same material and filled with the same fluid, difficulty was experienced in obtaining by direct measurement a sufficiently accurate value of d . To meet this, d was determined indirectly from the pitch. By (4) we have

$$\frac{p_0^2}{p^2} = 1 + \frac{n^4-n^2}{(n^2+1)^{\frac{3}{2}}} \frac{\rho B_0^{\frac{1}{2}}}{\sigma^{\frac{3}{2}} p_0 a}, \quad (18)$$

from which it appears that G^2-1 is inversely proportional to the pitch (before filling), as well as inversely proportional to the radius of the cylinder. In Auerbach's notation a constant C is employed, whose value for the case $n=2$ would be by (18)

$$C = \frac{6}{5^{\frac{3}{2}} \pi} \frac{\rho B_0^{\frac{1}{2}}}{\sigma^{\frac{3}{2}} a}. \quad (19)$$

In actual experiment the two-dimensional character of the fluid motion is disturbed by the existence of a free surface at

* Wied. Ann. Bd. xvii. p. 964.

which a special condition must be satisfied. Hence arises a vertical motion of the surface, which is the proximate cause of the "crispations" usually to be observed under these circumstances. In considering this question we may leave the force of gravity out of account, inasmuch as the period of free waves of length comparable with the diameter of the cylinder is much greater than that of the actual motion.

In accordance with (5), if the fluid be treated as incompressible, we may take

$$\phi = \alpha_n \cos pt \cos n\theta r^n + \Sigma A_\kappa \cos pt \cos n\theta e^{-\kappa z} J_n(\kappa r), \quad (20)$$

in which z is measured downwards from the surface, and κ denotes a root of

$$J'_n(\kappa a) = 0. \quad (21)$$

The coefficients A_κ are to be determined by the condition at the surface, which is simply $\phi = 0$. Thus for each value of κ

$$\alpha_n \int_0^a r^{n+1} J_n(\kappa r) dr + A_\kappa \int_0^a J_n^2(\kappa r) r dr = 0. \quad (22)$$

Now (see 'Theory of Sound,' §§ 203, 332)

$$\begin{aligned} \int_0^a r^{n+1} J_n(\kappa r) dr &= \frac{n a^n}{\kappa^2} J_n(\kappa a), \\ \int_0^a J_n^2(\kappa r) r dr &= \frac{1}{2} a^2 \left(1 - \frac{n^2}{\kappa^2 a^2} \right) J_n^2(\kappa a), \end{aligned}$$

so that

$$\phi = \alpha_n \cos pt \cos n\theta \left\{ r^n - 2n a^n \Sigma \frac{e^{-\kappa z} J_n(\kappa r)}{(\kappa^2 a^2 - n^2) J_n(\kappa a)} \right\}. \quad (23)$$

To calculate the kinetic energy we have to integrate $\phi d\phi/dn$ over the whole boundary of the fluid. Now at the free surface $\phi = 0$, and at a great depth the motion becomes two-dimensional. We have therefore only to consider the cylindrical surface. By supposition $J'_n(\kappa a) = 0$, and thus

$$\frac{d\phi}{dn} = \frac{d\phi}{dr} = n \alpha_n a^{n-1} \cos pt \cos n\theta.$$

We get therefore

$$\begin{aligned} T &= \frac{1}{2} \rho \alpha_n^2 n a^{2n} \cos^2 pt \iint \left\{ 1 - 2n \Sigma \frac{e^{-\kappa z}}{\kappa^2 a^2 - n^2} \right\} \cos^2 n\theta d\theta dz \\ &= \frac{1}{2} \pi \rho \alpha_n^2 n a^{2n} \cos^2 pt \left\{ z - 2n \Sigma \frac{\kappa^{-1}}{\kappa^2 a^2 - n^2} \right\}. \quad (24) \end{aligned}$$

The value of T is less than if the motion were strictly two-

dimensional by a quantity corresponding to the length

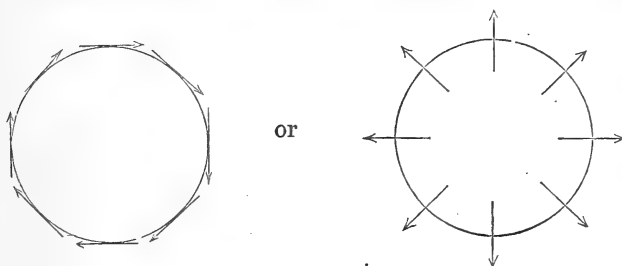
$$2na \sum \frac{\kappa^{-1}a^{-1}}{\kappa^2 a^2 - n^2} \dots \dots \dots (25)$$

For $n=2$, the values of κa from (21) are 3.054, 6.705, 9.965, 13.1, 16.3, &c.; and thus (25) becomes $.2674 a$.

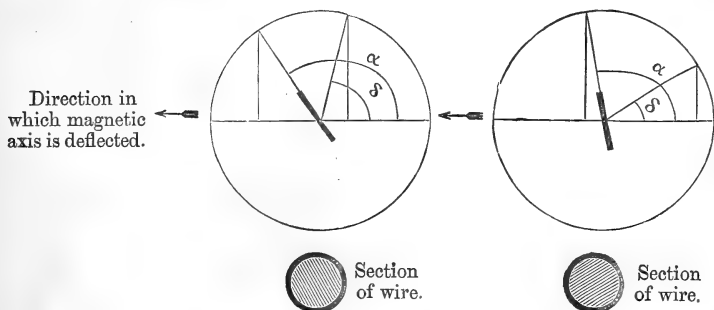
March 1883.

LVII. *A Method of Calculating the Amount of Magnetism of a Magnetic Circle for each Strength of Current acting on it.*
By W. MOON*.

IN unmagnetized iron the magnetic axes of the molecules lie indifferently in every direction; and this is equivalent to their forming small closed circuits, the axes of the two opposite molecules being opposed in direction. An electric



current acting upon a group of molecules tends to direct them to a certain position with respect to the current. The current exerts an equal force upon all molecules at the same distance from it; but since the molecules in their normal positions form different angles varying from 0° to 180° with current, some of the molecules would be deflected through a greater distance than others.



* Communicated by the Author.

Let δ equal the angle the molecule normally forms with the current, and let α equal the angle to which the current deflects the molecule. Then the power of the current to deflect the molecule will be equal to $\sin \alpha$, and the force tending to draw the molecule back to its normal position will be equal to

$$\text{versin } \alpha - \text{versin } \delta = \cos \delta - \cos \alpha;$$

but since the action of the current on each molecule is equal, therefore $\frac{\cos \delta - \cos \alpha}{\sin \alpha}$ will be the same with every molecule, whatever may be the value of δ .

Since the action of the current on the iron is proportional to the current-strength,

$$\therefore a = \mu \frac{\cos \delta - \cos \alpha}{\sin \alpha},$$

where a is current in ampères and μ a constant.

The amount of magnetism of the group of molecules is equal to the summation of the different distances through which the molecules are deflected

$$= \mu \int_{\delta=180^{\circ}}^{\delta=0^{\circ}} (\cos \delta - \cos \alpha) d\delta.$$

The values of $\cos \alpha$ corresponding to the different values of $\cos \delta$ from 0° to 180° in each value of $\frac{\cos \delta - \cos \alpha}{\sin \alpha}$ may be calculated by the following series:—

$$\text{Since } a = \mu \frac{\cos \delta - \cos \alpha}{\sin \alpha},$$

$$\therefore \cos \alpha = \cos \delta - \frac{a}{\mu} \sin \alpha,$$

$$\therefore \cos \alpha = \cos \delta - \frac{a}{\mu} \sqrt{1 - \cos^2 \alpha},$$

$$\therefore \cos \alpha = \cos \delta - \frac{a}{\mu} \sqrt{1 - {}^2 \left\{ \cos \delta - \frac{a}{\mu} \sqrt{1 - {}^2 \{ \cos \delta \dots \right.}}$$

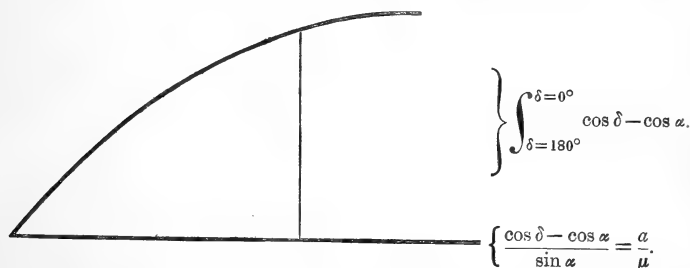
and the magnetism of a molecule

$$= \cos \delta - \cos \alpha = \frac{a}{\mu} \sqrt{1 - {}^2 \left\{ \cos \delta - \frac{a}{\mu} \sqrt{1 - {}^2 \{ \cos \delta \dots \right.};$$

and this series, integrated for all values of δ between 0° and 180° , would give the amount of magnetism (for any current-strength a) of a group of molecules that would in their normal position form a closed magnetic circle.

If the constant μ is made equal to unity, and a number of

values of $\int_{\delta=180^{\circ}}^{\delta=0^{\circ}} \cos \delta - \cos \alpha$ corresponding to each value of $\frac{\cos \delta - \cos \alpha}{\sin \alpha}$ were calculated, until each increment of $\int_{\delta=180^{\circ}}^{\delta=0^{\circ}} \cos \delta - \cos \alpha$ became very small as compared with each increment of $\frac{\cos \delta - \cos \alpha}{\sin \alpha}$, it would be found that the different values of $\int_{\delta=180^{\circ}}^{\delta=0^{\circ}} \cos \delta - \cos \alpha$ form the ordinates of a curve which approaches towards an asymptote,—the abscissæ representing the current-strengths, and the ordinates the amount



of magnetism of a magnetic circle of molecules—that is, the intensity of magnetism of the iron.

LVIII. *Dissymmetry in the Electrolytic Discharge.* By ALFRED TRIBE, *F.Inst.C., Lecturer on Chemistry in Dulwich College**.

IN the Philosophical Magazine for June 1881, p. 446, I gave the results of some experiments, from which it was concluded that the electrical discharge through an electrolyte whose transverse section was greater than the width of the electrodes was accompanied by dissymmetry in opposite but corresponding parts of the field of electrolytic action. The evidence consisted in the very different superficial magnitudes of the electrifications registered on similar plates of silver in corresponding positions near the + and — electrodes respectively. The silver plates, which I have called *analyzers*, were immersed in a solution of copper sulphate undergoing electrolysis. It was likewise shown that the dissymmetry was connected with the spreading-out of the lines of force; for as this was prevented, so the dissymmetry was less and less marked;

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and when the lines crossed the field parallel to one another, what I have regarded as evidence of dissymmetry was no longer observed.

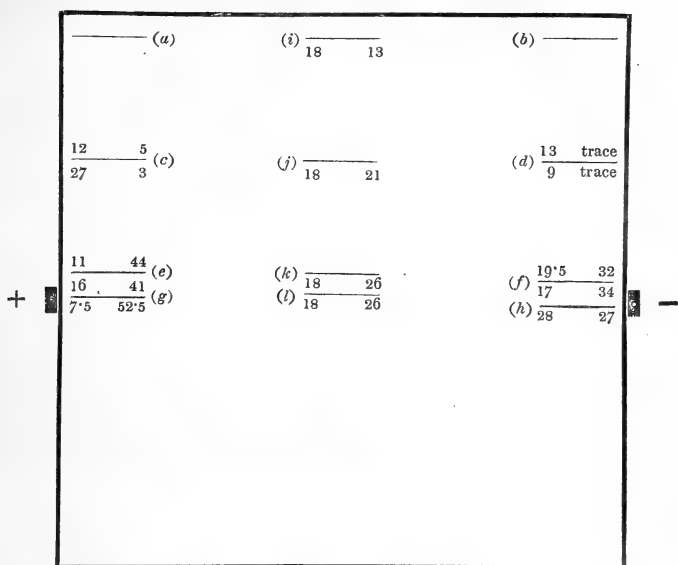
I had hoped to work at several questions suggested by these results; but as I am unable at present to proceed with the experimental development of the investigation, I propose in this communication to record without further delay my unpublished observations bearing more especially on the subject of dissymmetry. This is the more advisable, in view of the interesting observations of Prof. Roiti (*Nuovo Cimento*, s. 3. vol. x.), and of the recent experimental and theoretical studies of Drs. Pasquolini and Volterra (*A. dell' Accad. Sc. Torino*, vol. xviii.).

In the first set of experiments to be described I employed a trough 305×305 millim. and 128 millim. deep, a 5-per-cent. solution of copper sulphate filling the trough to within 8 millim. of the top; *copper electrodes* about 7.5 millim. wide, placed in grooves running down the centre of two opposite sides of the vessel; *analyzers* of fine silver, 67×7 millim. (weighing about .75 gram), placed lengthwise, midway between the surface of the solution and the bottom of the cell, and parallel with a vertical plane joining the electrodes. The analyzers were supported by means of a small wedge of the metal projecting from the lower edge at the centre of the plates, this being fixed into a small stand of paraffined wood. In each experiment one analyzer was employed and a current of two amperes for six minutes.

The numerical results of twelve experiments are given in the annexed horizontal section (fig. 1), showing the position occupied by the several analyzers. It will be remembered that the + ion (copper) and the peroxide of silver form permanent images of the - and + electrifications respectively, so that after the completion of an experiment the magnitudes of these electrifications can be measured. It will also be remembered that the + ion is deposited on that end of the analyzer nearest the + electrode, or the side of the cell in which this electrode is fixed, and that the grey silver peroxide is formed on that part of the analyzer nearest the - electrode, the two effects being separated by a part of the plate on which no visible action has taken place. The numbers give the lengths of the electrifications in millimetres.

In the cases where they were the same on the two sides of the analyzer, numbers are placed on the one side only in the diagram. Of course the numbers at the ends nearest the + electrode show -, while those at the ends nearest the - electrode refer to + electrifications.

Fig. 1.



The electrochemical actions on the analyzers *a, b* were too small to allow of a definite opinion being formed as to dissymmetry in the parts of the field surrounding them; but in the parts of the field *c, d, e, f,* and *g, h,* dissymmetry was most distinctly indicated by differences in the magnitudes of the electrification of the same sign on the several analyzers in opposite but corresponding positions. Taking the corresponding parts of the field where the greatest dissymmetry would appear to exist, the negative electrifications on *g* and *h* were in the proportion of 1 : 3.6, while the + on the same plates were as 1.9 : 1.

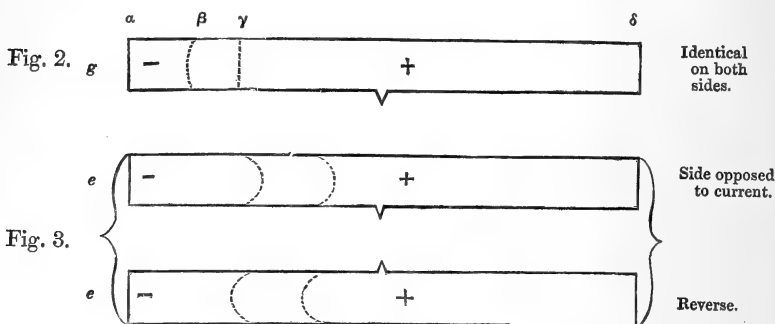
In connexion with these experiments, I would note one or two other observations not directly connected with dissymmetry. The relation which I have already shown to exist (Proc. Roy. Soc. 1881, p. 435) between the shape of the boundaries of the electrochemical deposits (*i. e.* where the chemical effects of the electrifications meet the unaffected surface of the analyzer) enables the directions of the lines of force to be determined in the several parts of an electrolytic field, unless it is very small in intensity. The lines across the field cut by the analyzers *g, l, h* were thus shown to be parallel with the longer edges or sides of the silver plates; and in the immediate neighbourhood of *i, j,* and *k* the lines were approximately in the same direction; but surrounding the plates *c, d, e,*

and f the curvilinear character of the lines was most distinctly shown to exist.

I would also direct attention to the magnitudes of the electrifications on the analyzers i, j, k . It will be observed that the $-$ was the same on each of the plates, while the $+$ varied from a lineal distribution of 13 to 26 millim. This would appear to show that the magnitudes at least of the $-$ electrifications bear little or no relation to the intensity of the field, as it is obvious considerable variations in this respect existed in the several parts of the field. Any generalization, however, to be of real value would require many more observations on this branch of the subject.

Another point is the variations in the relative magnitudes of the opposite electrifications. In eight of the cases where the effects of both electrifications were sufficiently marked to allow of measurement, the $+$ were greater on both or one side of the analyzer than were the $-$ on the same side or sides; but in four cases the $-$ were greater than the $+$.

Figs. 2 and 3 are diagrams of the plates g and e (fig. 1). The dotted lines indicate the shape of the boundaries, and the spaces enclosed between one of these lines and the corresponding end of the analyzer, the magnitudes of the respective electrifications; α to β being the $-$, β to γ the part of non-electrification, and γ to δ the $+$.



II. *Dissymmetry by Interference.*—I now describe some experiments made in the hope of throwing light on the subject of this paper. The results I do not offer as an explanation. I record them in this place, first, for the interest which belongs to them apart from other considerations; secondly, they may perhaps serve as the starting-point for fresh inquiry.

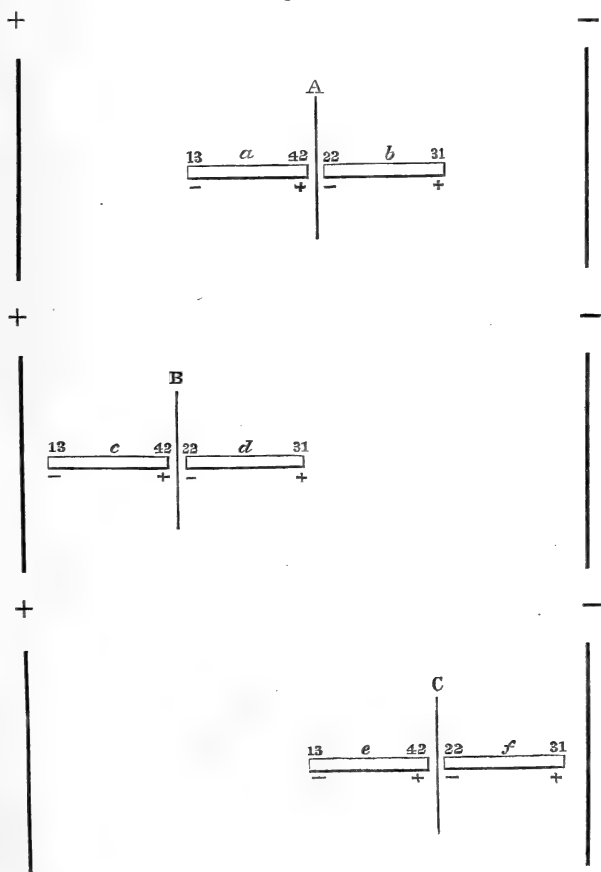
A trough was employed 305 millim. long, 120 broad, and 128 deep; and copper electrodes of the breadth and depth of the cell placed at its respective ends. The strength of copper-

sulphate solution, dimensions &c. of analyzer, were the same as used in the previous experiments; but a current of one ampère was employed.

An ebonite plate 76×76 millim., about $1\frac{1}{2}$ millim. in thickness, was placed parallel to the electrodes and in the centre of the liquid. In the position A shown in the vertical section (fig. 4) it was midway between the electrodes; in B, 71 millim. from the + electrode; and in C, 71 millim. from the - electrode. Analyzers *a*, *b*, *c*, *d*, *e*, *f* were successively placed lengthwise 2 millim. from, and perpendicular to, the centre of the plates.

The numbers in the diagram refer, as before, to the electrifications on the several analyzers.

Fig. 4.



I might be allowed to point out that in a simple uniform field, such as would obtain were the ebonite plates absent, the magnitudes of like electrifications on analyzers in the positions indicated would have been absolutely identical. It will be observed, therefore, that the ebonite plates exerted an appreciable disturbing influence. It was of course to be foreseen that the direction of the lines of force would be changed, and the quantity of current flowing in the immediate neighbourhood of the analyzers would be diminished by the interposition of such a nonconducting plate; but the dissimilarity in the magnitudes of like electrifications, on the analyzers in the corresponding parts of the field relatively to the respective electrodes and sides of the plate, would appear to point to the existence of dissymmetry in those particular parts of the field, and, moreover, to show that this dissymmetry was produced by interference with the transmission of electricity by the electrolyte.

It was to be anticipated that, on reducing the amount of this interference, the dissymmetry would be reduced. Such was found to be the case. On taking a smaller nonconducting plate the dissymmetry became less marked.

I would now point out, first, that the general effect of interposing a nonconducting plate as regards symmetry would appear to be similar to that produced by the spreading-out of the lines of force; secondly, that the same extent of dissymmetry existed near the sides of the plate in very different parts of the field. This last fact shows that the phenomenon is independent of any direct action of the electrodes, attractive or otherwise.

In conclusion, I would remark that the dissymmetry pointed out in this paper is probably but a fresh example of the dissymmetry so common in ordinary electric discharges, and that the electrolytic field appears to possess advantages for the more complete examination of this very important subject. That the dissymmetry was real and primary, and not an apparent difference brought about by secondary action of the plates, would appear from the observations that the differences in the magnitudes of the like electrifications were found in corresponding parts of the field *only* when the lines of force were thrown out of their more direct course, and also from the observations that differences were found immediately the current passed into the electrolyte, when plates were used singly or together, when copper analyzers were employed and when the analyzers were of platinum (though in this case the — electrifications only could be observed).

April 30, 1883.

LIX. *On winding Electromagnets.* By Professors W. E. AYRTON, *F.R.S.*, and JOHN PERRY, *M.E.**

[Plates VIII. & IX.]

THE following experiments were made to determine which mode of winding a given length of wire on an iron bar gave the strongest electromagnet for the same current. Four bars of iron, each 12 inches long, were cut from the same rod $\frac{3}{8}$ inch thick; and an exactly equal length of wire was wound on the four bars respectively, in the following way:—

1. Wire wound equally over the whole length (Pl. VIII. fig. 1).
2. Wire coned towards each end (fig. 2).
3. Wire wound equally over half the iron bar, leaving the other end bare (fig. 3).
4. Wire wound on one half but coned towards the end (fig. 4).

Electromagnet No. 1 was put so that its axis was at right angles to the axis of a small magnetic needle and passed through the point of suspension of the needle, which was suspended so as to move freely in a horizontal plane, and far enough away that the magnetic field due to the electromagnet No. 1, when magnetized by passing a current through it, was nearly constant over that portion of the field in which the little suspended needle moved when deflected. A constant current was now passed through the coil on No. 1, and the deflection of the little needle observed when No. 1 was placed at different distances from the centre of the test-needle, the axis of No. 1, however, always remaining in the same line. Under these circumstances it is well known that the strength of the field produced by No. 1 at the centre of the test-needle is approximately proportional to the tangent of its deflection. Experiments were now made in a similar way with electromagnet No. 2, and with each end of No. 3 and of No. 4, the same current as was employed with electromagnet No. 1 being used in all cases, and which was much below the saturating current.

The results obtained are given in the accompanying table, and are shown plotted in the accompanying curves (fig. 5), vertical distances representing the distance between the near end of the electromagnet and the centre of the test-needle, and horizontal distances the tangents of the deflection of the test-needle: A A A A is that for No. 1; B B B B for No. 2; C C C C for the covered end of No. 3; D D D D for the uncovered end of No. 3; E E E E for the covered end of No. 4; and F F F F for the uncovered end of No. 4.

* Communicated by the Physical Society of London; read December 9, 1882.

Distance in inches between the near end of the bar and the centre of the test-needle.	No. 1.		No. 2.		No. 3.				No. 4.			
					Covered end.		Bare end.		Covered end.		Bare end.	
	Def.	Tan.	Def.	Tan.	Def.	Tan.	Def.	Tan.	Def.	Tan.	Def.	Tan.
3½.....	79°	5.14	77°	4.33	82°	7.12	57°	1.54	67°	2.30	27°	0.57
4	77	4.33	71	2.9	77	4.33	53	1.33	62	1.88	21	0.38
5	69½	2.67	58	1.6	66	2.24	46	1.04	52	1.28	14	0.28
6	59	1.66	47	1.07	56	1.48	39	0.81	43	0.93	11	0.19
7	50	1.19	37	0.76	46	1.04	32	0.62	36	0.73	9	0.16
8	42	0.9	30	0.58	37½	0.79	27½	0.52	29	0.56	7	0.12
9	35	0.7	24	0.46	30	0.58	18	0.32	22	0.48	4	0.07
10	30	0.58	20	0.36	25	0.47	13	0.23	17	0.31	3	0.05

To ascertain the distribution of the lines of force, iron filings were sprinkled on paraffined paper, and the positions the filings took up fixed by the paraffin being softened by a heated piece of copper being passed over the paper at a short distance above it. These fields of force are shown in the diagrams 6, 7, 8, and 9(Pl. IX.). From the curves in fig. 5 and from the iron-filing curves it is seen that the effect of coning the wire is to produce a strong field very near the pole, but that the force falls off very rapidly as the distance from the pole increases. With No. 2 magnet, for instance, the field between the poles is so weak that scarcely any definite arrangement of filings is traceable in the diagram 7 corresponding with it.

From the curves in fig. 5 it is seen that, at considerable distances from the end of the electromagnet, the uniformly coiled magnet No. 1 produces the most powerful field, while for points nearer the magnet, but still at a distance of about 3 inches from it, the covered end of No. 3 magnet, corresponding with the curve C C C, produces the strongest field, the next strongest being produced by the magnet No. 2 with the wire coned towards each end, since obviously the curve B B B cuts the curve A A A at a point corresponding with a distance of about 3 inches from the end of the magnet. For distances very close to the magnet, this method of experimenting cannot, of course, be employed to measure the resultant force accurately; and hence observations by this method at distances of less than 3½ inches from the end of the magnet to the centre of the oscillating needle were not made, and conclusions as to the resultant magnetic force very close to the poles must, of course, not be drawn from the curves in fig. 4.

Returning to the curves taken up by the iron filings, we see that No. 1 magnet gives an arrangement similar to that

of an ordinary regularly magnetized bar-magnet. With No. 2 the lines around the poles are similar to those of No. 1, but the field between the poles is very weak. Magnets Nos. 3 and 4 give very similar figures, showing a very peculiar distribution of force. There is a great concentration of the lines at the pole corresponding to the half of the iron which is covered with wire ; but the unwound end seems to form a long weak pole, with its maximum force near the centre of the bar, *i. e.* at the inner end of the coil,—the differences between these two being, that with No. 4 magnet there is, comparatively, a greater concentration of force at the wound pole, and that the opposite pole is longer and extends a little way into the coil—the result of the coning of the wire. In these two cases the unwound end of the iron seems to act like an armature.

To ascertain the force which each magnet would exert on an armature, experiments were made and the following results obtained, the current flowing through the coil in each case being exactly the same, as well as the armature employed :—

Magnet.	Weight required to detach the armature from the covered end of the magnet.
No. 1	45 ounces.
2	57 "
3	57 "
4	77 "

These results confirm those previously obtained, that the field produced by the covered ends of the electromagnets numbers 2 or 3 at distances near the pole is much stronger than that produced by No. 1. But they show something else, *viz.* that for very small distances it is the covered end of No. 4 that produces the strongest field. In other words, returning to fig. 5, the curve *EE*, although much below the curves *AA*, *BB*, and *CC*, must rise rapidly and cut the others, just as the curve *CC* cuts the curve *AA*, at a point corresponding with a distance of about 4·2 inches from the end of the magnet, and just as, again, the curve *BB* cuts *AA* at a point corresponding with a distance of about 3·2 inches from the end of the magnet. The curves of iron-filings (fig. 9) indeed give indication of the great strength and concentration of field there is produced close to the iron by the wire coned at the end, as employed in the magnet No. 4.

With, then, a definite iron core, a definite length of wire to be coiled on it, and to be traversed with a definite current, the mode of coiling to produce the largest field depends entirely on the distance from the end of the electromagnet at which

the field is to be produced. With the particular magnet we have employed we see that, at distances from the end of the magnet very small compared with the length of the core, the wire should all be coiled up at the near end of the core, as in fig. 4; to create a field at a distance from the end of the magnet equal to about a third of the length of the magnet, it is better to coil the wire uniformly over one half of the core, as in fig. 2, than to cone it up at the near end as in 4; while for distances from the end of the magnet equal to, or greater than, about $\frac{4}{5}$ of the length of the core, the uniform mode of winding is the best.

We have to thank two of our students, Messrs. Sayers and Pink, for most cordial assistance rendered us in this investigation.

LX. *The Regenerative Theory of Solar Action.* By ERNEST H. COOK, B.Sc. (Lond.), A.R.C.S., Trade and Mining School, Bristol*.

I HAVE ventured thus to name the theory recently propounded by Dr. (now Sir Wm.) C. W. Siemens, in a paper read before the Royal Society in March 1882, because of the essential feature which it possesses. Solar heat is kept up, on this theory, by supposing reciprocal actions to go on using only the same materials but under different conditions. The different conditions are those which obtain in or near the sun and those of interplanetary space—in the first case subjecting the matter to intense heat and pressure, in the second to intense cold and rarefaction. The theory has received considerable discussion, more especially by MM. Faye and Hirn (*Comptes Rendus*, Oct. 9 and Nov. 6, 1882); but there are some points which, so far as I am aware, have not been raised. As the theory possesses a very high scientific value, and as Dr. Siemens, in enunciating it, states that he submits it “to the touchstone of scientific criticism,” I venture to point out what appears one or two difficulties connected with it. If another reason for presenting these points for consideration be wanted, it is in the hope that it will give Dr. Siemens the opportunity of making some such interesting communication as he has done in his reply to the criticism of M. Faye (*Comptes Rendus*, Oct. 30, 1882)†.

* Communicated by the Author.

† The original paper and the whole of the published criticisms which it has called forth, together with the replies of Dr. Siemens and other matter, are contained in a volume recently published by Macmillan & Co., entitled ‘The Conservation of Solar Energy.’

The fundamental assumption of the existence of a universal atmosphere, to which M. Faye so strongly objects, is one which, it seems to us, both theoretical and experimental evidence supports. But the deductions which are drawn from this we do not think can be supported. It is supposed that the atmospheres attracted by the planets would consist for the most part of the "heavier and less diffusible gases—nitrogen, oxygen, and carbonic anhydride; whilst hydrogen and its compounds would predominate in space." But if such a partial separation takes place, we should expect that the heaviest constituent of the universal atmosphere (viz. the carbonic anhydride) would exist in larger quantity in the planetary atmospheres. More especially should we do this when we remember the great difference in the relative density of CO_2 and O and N. Also, if such a selective attraction takes place, we ought to find a slight difference in the composition of the air at high altitudes and at low ones. Saussure, who has investigated this point, states that at high altitudes the proportion of dioxide is generally greater than at lower levels (*Pogg. Ann.* xix. p. 391). Among other investigators we may mention "Gay Lussac and Thénard, who collected air in a balloon at an elevation of 7000 mètres, and found it to contain exactly the same proportional quantity of oxygen as that collected at the same time in Paris and analyzed in the same way. Their results have since been corroborated by the more exact investigations of Brunner, who analyzed the air collected at the top and at the bottom of the Faulhorn, and found in each case exactly the same proportion between the oxygen and the nitrogen"*.

Also Frankland (*Chem. Soc. Journ.* xiii. p. 22) has found that the composition of the air up to altitudes of 14,000 feet is constant. We may thus conclude that, so far as terrestrial observation goes, there is absolutely no evidence supporting the partial separation of the constituents of the atmosphere supposed to take place.

Dr. Siemens cites the composition of the gases found occluded in meteorites as supporting his view of the existence of an atmosphere very rich in hydrogen existing in space. There is also a general belief that the presence of these gases under pressure proves that they have passed through a region where hydrogen exists under a much greater pressure than that of our own atmosphere. According to this view the gas contained in the body is really a portion of the interplanetary atmosphere; it ought therefore to consist largely of hydrogen, while the proportion of the heavier gases should be much smaller than that existing in the air. But we find, on the

* Roscoe and Schorlemmer, 'Chemistry,' vol. i. p. 440.

contrary, that in the analysis quoted* there is no less than *four* times as much carbon dioxide as in ordinary atmospheric air. This particular analysis is moreover somewhat favourable than otherwise, inasmuch as some meteors contain a much larger quantity of CO_2 . Thus Dr. A. W. Wright (*Amer. Journ. of Sci.* ix. p. 459) found as much as 35 per cent., and J. L. Smith (*Chemical News*, xxxii. p. 221) found 13·03 per cent. of this gas. If, therefore, these bodies derive their gaseous constituents from the interplanetary atmosphere, it must be one very rich in the heavy gas carbon dioxide. But I imagine that a very much easier explanation of the presence of hydrogen and the lighter gases can be given. As I am not aware that such an explanation has been given before, it will be necessary to state it rather fully.

In a very full and complete memoir by F. Mohr (*Liebig's Annalen*, clxxix. pp. 257–282), in which the author discusses the origin of meteorites, it is proved that they possess a porous structure. By heating in a stream of dry carbonic-acid gas and collecting the water in anhydrous cupric sulphate, it is proved that in the pores of these bodies water is contained. Out of seven meteorites examined an average quantity of water of ·856 per cent (·285 to 1·43) was found. Also the author concludes from experiment that organic substances analogous to ozokerite exist in them. Lawrence Smith (*Comptes Rendus*, lxxxii. pp. 1041 & 1507) has also shown the presence of carbonaceous bodies in meteoric masses. It is also well known how largely iron enters into the composition of these substances. Such bodies come into contact with our atmosphere and are immediately raised to a high temperature—a temperature sufficient to melt their outer surface, and which Mohr (*loc. cit.*) has been enabled to imitate by exposing them to the oxyhydrogen-flame. The first effect of this heat will be to decompose the organic materials, which will really be subject to a process of destructive distillation, one result of such decomposition being the production of marsh-gas. The carbon will either be set free or will combine with oxygen to form carbonic anhydride or carbonic oxide according to the amount of the latter element present. This oxygen would be obtained in this way:—if an oxide of iron is present, the organic matter would probably reduce some of it to either the ferrous condition or to the metallic state; and it is in these conditions that we find the iron to exist. But the larger portion of the oxygen and the greater part of the hydrogen (some of this would probably be obtained by the decomposition of the organic matter, just as we find it thus produced in coal-gas) would be pro-

* Dr. Flight's.

duced from the decomposition of the water. When heated in contact with iron, water is readily decomposed, the whole of its hydrogen being set free and the oxygen combining to form magnetic oxide. Magnetic oxide is accordingly found existing in meteoric bodies. Some of this oxide would, however, be reduced in the way before mentioned, and thus the major part would consist of iron. The fused shell of material surrounding the body would prevent the escape of these gases, which would thus exist under a state of pressure. There can be no doubt that the volume of hydrogen thus produced would be many times greater than the volume of the body; for, taking Mohr's figures and assuming the average specific gravity to be only 3, we find that the volume of hydrogen would be *more than thirty times that of the meteor containing it*. An objection to this theory is, that the time during which the meteor is heated is so short that the changes indicated would not be likely to occur. This is simply a matter for experiment. It may, however, be mentioned that, supposing the body to fall from rest through the atmosphere (supposed for the present purpose to be 50 miles high) to the earth, it would take about 2 minutes in the descent. But the meteor which falls to the earth today may have on some previous occasion passed through the atmosphere of our own or some other planet, and been heated by the passage. The evidence therefore afforded by these bodies may not really be of much importance; in fact, if the above view be accepted, it must be admitted that they afford us no insight into the composition of the supposed interplanetary atmosphere.

The prime cause of the movement of the cosmical atmosphere is the rotation of the sun upon its axis. But all the planets rotate also; and as they are all immersed in the same atmosphere we shall have the same action occurring in their case—*i. e.* a drawing-in towards the poles and a propulsion at the equator. Thus we ought to find an aerial current flowing constantly in the northern hemisphere from the north-east (allowing for the gradually increasing velocity of rotation), and in the southern hemisphere from the south-east. As these currents would occur in the higher regions of the atmosphere, they would be exactly opposed to the "return trades" which are prevalent there. This is a consequence of the theory which may have been overlooked.

If there is one fact more than another upon which scientific men are agreed, it is upon the existence in the sun of metallic vapours. These Dr. Siemens considers to form an inner atmosphere surrounding the nucleus (?). Supposing this theory of solar action to be a correct one, and that dissocia-

tion of the CO_2 and H_2O does not occur, we have at the bounding surfaces at any rate, and, as is admitted, sometimes far down into this inner atmosphere, a large mass of water-vapour and carbon dioxide in contact with metallic vapours. Here, then, surely combination will occur between the metals and the oxygen of water, setting free the hydrogen. Large quantities of the oxides of sodium, potassium, calcium, magnesium, which it is impossible to decompose at any terrestrial temperature, would thus be produced. Most of the oxygen would thus be retained in the sun itself; and thus the constitution of the gases projected into space would be very different from that supposed by Dr. Siemens. The comparison of the solar spectrum with the spectra of these metals seems to conclusively prove that the bodies existing in the sun are the elements themselves and not any compound of them. This being so, it shows that the temperature is higher than that of the dissociation of these oxides; but if higher than this, surely it must be very much higher than that at which such bodies as carbon dioxide and water can exist.

Referring to the metallic vapours revealed to us by the spectroscope, Dr. Siemens says:—"These form a lower and denser solar atmosphere, not participating in the fan-like action which is supposed to affect the light outer atmosphere only, in which hydrogen is the principal factor." This assumption is difficult of acceptance, inasmuch as any force which affects one body will also affect another, even though they differ in density. It is simply a question of degree; and if, owing to centrifugal force, hydrogen is projected x miles into space, then sodium-vapour, which is 23 times as heavy, will be projected $\frac{x}{23}$ miles. Again, we are told carbon dioxide

and water are formed and projected into space, but in consequence of the greater density of the materials composing the inner atmosphere they do not suffer propulsion. But here we are in error. A large number of the metallic vapours have densities which are very little greater than the density of CO_2 ; and some are absolutely less. Thus the specific gravity of carbon dioxide compared with hydrogen is 22, that of lithium-vapour is only 7, of beryllium 9.2, of sodium 23, magnesium 24, and aluminium 27. All these have been proved to exist in the sun. Since they are specifically lighter than the supposed products of combustion, and are also exposed to the same heat, and therefore expanded just as much, it is unreasonable to suppose they are not affected in the same way by the action of the centrifugal force. If, however, we imagine these metals to combine with oxygen, we are in no way better off; for here again the products are very little heavier than the

CO₂. The specific gravity of the vapour of lithic oxide would be 15, that of beryllia probably 12·6, of magnesia 20, of sodic oxide 31. According to these considerations the various substances would be projected to the following proportional distances:—

	Metal.	Oxide.
Aqueous vapour	100	100
Carbon dioxide	41	41
Lithium	129	60
Beryllium	98	71
Sodium	39	29
Magnesium	37	45
Aluminium	33	18
Calcium	22	32

It would thus appear that the composition of the stellar atmosphere must be far more complicated than is supposed, and the possible existence of an inner atmosphere seems problematical.

There is only one other point to refer to. The researches of spectroscopy and the revelations of the telescope have revealed to us the fact that our sun is only one of an innumerable number of similar bodies. A theory, then, to be complete, ought to account for the action of all. Moreover Dr. Huggins has shown that the fixed stars may be divided into classes according to the spectra which they emit. Thus we have every gradation, from the spectrum of a white or bluish-white star like Sirius, passing through a yellowish one like our sun, up to a reddish one like Arcturus. Now, since we have the same atmosphere supplying all and the same cause producing motion, it is difficult to see how these differences are to be accounted for. Dr. Huggins has supposed these observations to show that the stars are of different ages—some being in the height of their energy and thus producing spectra in which the shorter waves predominate, while others have passed this stage and are yielding radiations of longer period. Dr. Siemens mentions that, as the whole solar system is moving through space at a very great velocity, the nature of the solar fuel may vary at different times, and thus a difference in the energy of the action may be caused. But any difference which may exist would be rapidly removed by the action of gaseous diffusion, which would rapidly cause the universal atmosphere to become practically homogeneous. Again, if the fuel varies from time to time, how is it that we find the spectra of the various stars to be constant? They would under these circumstances, we are inclined to imagine, present none of those constant characters which Dr. Huggins has established.

LXI. *Experiments on the Viscosity of a Solution of Saponine.*
 By W. H. STABLES and A. E. WILSON, Yorkshire College,
 Leeds*.

M. PLATEAU has shown (*Statique des Liquides*, t. 2, ch. vii.) that a body placed in a liquid and wetted on one side only, experiences in many cases a greater resistance to its motion than if it were completely immersed. Some controversy has arisen between Marangoni and himself as to the cause of this phenomenon, which M. Plateau explains by the assumption that the liquids in question possess a surface viscosity greater than that of the interior. The liquid in which the surface resistance is in most striking contrast to that of the interior is a solution of saponine in water.

Oberbeck (Wiedemann's *Annalen*, Bd. 11, S. 634) has repeated and extended Plateau's experiments, using an oscillating disk instead of a magnetic needle. He made no observations upon saponine solution. The object, therefore, of the following investigation is to study the movements of a disk when oscillating in or near the surface of a solution of this substance.

Oberbeck found that the resistance of a water-surface increased largely with exposure to the air; but he also proved that even with fresh distilled water the resistance is considerable. As he points out, we are therefore led to one of two conclusions—namely, that either water has a surface viscosity different from that of the interior, or else that a pure water surface cannot be obtained.

The apparatus used in our experiments was similar to that employed by Grottrian (Pogg. *Ann.* Bd. 157, S. 237).

It is fully described by him; and it is therefore unnecessary to figure here the connexions of the different parts. The diagrammatic representation in fig. I. may, however, conduce to clearness.

It consists of a circular disk A of nickel-plated brass 76·25 millim. in diameter and 2 millim. thick. In the centre is screwed a brass rod, to which a concave mirror B and a small iron bar C (used to set the apparatus in motion by means of a magnet) are attached. A wire (119·8 centim. long), employed to put the apparatus in motion by torsion, is firmly gripped between two small plates at D.

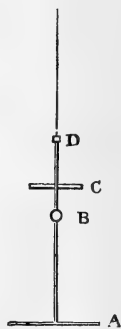


Fig. I.

* Communicated by the Physical Society, having been read at the Meeting held on April 14, 1883.

The brass rod by which the plate was suspended could be unscrewed and replaced by others of different sizes.

The plate was suspended in the centre of a circular glass dish 15.5 centim. in diameter and 8 centim. deep. This was fitted with a wooden cover, through which a thermometer was introduced. The whole was surrounded by a case having glass sides, which served to ward off air-currents. The time of the oscillations was measured by a stop-watch indicating quarter-seconds.

Three rods were used in turn to suspend the plate. They were of the same length, but of different radii.

The radius of (1) was .1337 centim.

„ (2) „ .2360 „

„ (3) „ .3362 „

The scale on which the beam of light was reflected was about 2.5 metres from the mirror; the maximum half-amplitude was about 200 millim.; so that the half-oscillation was about 2°.3.

The moment of inertia of the apparatus was determined by means of a brass ring of the same external diameter as the plate, and was found to be 753.09 (C.G.S.).

The plate was first carefully levelled, and distilled water poured into the vessel to within a few millimetres of the bottom of the plate. Water was then added with a pipette until the bottom of the plate and the surface of the water touched.

By a simple calculation the difference in height caused by the addition of a measured quantity of water was determined. An addition of 2 cubic centim. of water was found to cause a rise of .1 millim. in the level.

The elevation was of course increased when the disk was in the surface; but the change is allowed for in the calculations.

The temperatures of the air and of the water were carefully noted at the commencement of each observation, and were found to remain nearly constant. To attain this end the experiments were conducted in a cellar, and special precautions were taken to prevent any marked variations in temperature. The variations therefore did not exceed 1° C.; and the small corrections thus rendered necessary were made by means of the table of the values of the coefficient of viscosity of water at different temperatures given by Grottrian (*Pogg. Ann.* Bd. 157, S. 242). All the observations were thus reduced to 16° as a standard temperature. The time of oscillation was ascertained by observing the time of 10 swings.

This operation was repeated a number of times and the mean taken. The logarithmic decrement was calculated from the readings obtained from the graduated scale.

The plate was suspended by each of the three rods in turn immersed to a depth exceeding 1 centim., and the logarithmic decrement and mean time of oscillation determined for each. The following table shows the results obtained with the saponine solution and with water:—

TABLE I.

Water.			Saponine.	
Rod.	Time of oscillation.	Log. dec.	Time of oscillation.	Log. dec.
	sec.		sec.	
1.....	5·19	·0477	5·24	·0785
2.....	5·20	·0479	5·26	·1424
3.....	5·22	·0483	5·26	·2045

The great difference between the surface-properties of saponine and those of water is here made very evident. The alterations in the dimensions of the rod which produced a slight effect only in the case of water increased the logarithmic decrement in the case of saponine two and a half times, a result which could only have been due to the increase of the section in contact with the surface.

If we assume that the disk oscillated under the influence of two forces, one of which (that of torsion) is proportional to the angular displacement from the position of rest, while the other, due to the viscosity of the liquid, is proportional to the velocity, the latter is measured by $M\lambda/T$, where M is the moment of inertia, λ the logarithmic decrement, and T the time of an oscillation. If, as in the case of a saponine solution, the surface resistance be so great that the friction between the surface layer and the interior may be neglected with regard to it, $M\lambda/T$ would be approximately of the form $a + br^2$, where r is the radius of the rod and a and b are constants.

The values of these, determined from the above equations by the method of least squares, are $a = 8·42$, $b = 191·6$.

Using these coefficients to calculate the value of $M\lambda/T$ from the observations on the saponine solution, we obtain the following results:—

TABLE II.

r .	$M\lambda/T$.	
	Observed.	Calculated.
0		8.42
.1337	11.27	11.85
.2360	20.37	19.07
.3362	29.36	30.07

The numbers are perhaps in as good agreement as could be expected, if we remember that the theory on which they are calculated is only approximate. If in the case of water we neglect b , the value of a is 6.92.

After the above preliminary observations a careful series of experiments was made in which the plate was gradually immersed to a greater depth in water.

The following are the results obtained :—

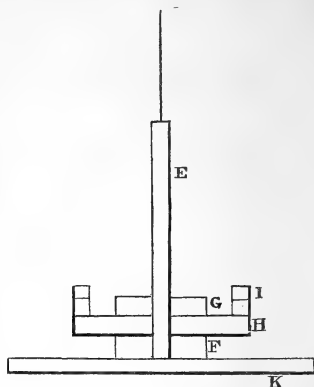
Position of plate.	Time of oscillation.	Log. decrement.
	sec.	
Upper edge of plate .7 mm. } above level of water	5.06	.0258
.56 millim. above	5.10	.0259
.28 " "	5.10	.0260
.14 " "	5.11	.0262
Top of plate level with sur- } face of water	5.10	.0266
.1 below	5.12	.0272
.2 "	5.11	.0272
.3 "	5.13	.0285
.4 "	5.17	.0298
.5 "	5.18	.0329
.7 "	5.20	.0366
.9 "	5.20	.0415
1.3 "	5.20	.0479
1.7 "	5.20	.0488

In the above results, the level of the liquid is corrected for the displacement of the liquid by the plate.

Similar experiments were next made on the saponine solution ; but it was at once seen that the apparatus used in the previous experiment with the water was quite unsuitable on account of the great resistance offered by the surface. Cer-

tain modifications were therefore introduced. In this second form (fig. II.) a steel piano-wire was used, the length of which was 98·35 centim. and the diameter 1·025 millim. Suspended by this wire was a stout brass rod, E, terminating in a disk, F, upon which rested a similar movable disk, G; and between the two disks could be fitted a brass plate, H, carrying heavy brass rings, I. To the bottom of the lower disk an (unmagnetized) steel bar K was attached, by means of which the apparatus was set in oscillation by the use of a magnet. The apparatus used in the water experiment was attached to the centre of this bar by a strip of metal attached to D (fig. I.) and soldered to the bar. Great care was taken to render the junction perfectly firm, so that no torsion could possibly take place at this point. The moment of inertia was determined as before, and found to be 186653 (C.G.S.).

Fig. II.



The first fact which was evident from the experiments was that, although the apparatus when suspended in water oscillated isochronously, it did not do so when suspended in the saponine solution. The following table gives the results of a number of experiments on the time of two long and two short swings respectively in that solution:—

Large amplitude.	Small amplitude.
10·90 sec.	10·28 sec.
10·70 "	10·28 "
10·75 "	10·35 "
10·52 "	10·35 "

With a thicker rod:—

10·42 sec.	9·85 sec.
10·52 "	9·73 "
10·33 "	9·90 "

Similar experiments in the case of water gave for two long oscillations, 10·25; for two short, 10·30; for three long oscillations, 15·5 and 15·75; and for three short, 15·6 and 15·75 seconds.

The amplitudes denoted long and short are not all of equal

size; and as those called "large" are much larger than those actually used in the experiments, the correction which might otherwise have been necessary has been neglected.

The following are the results of the experiments on saponine solution:—

Position of plate. surface	Upper	Time of oscillation.	Log. decrement.
·14 millim. above liquid...		9·51 sec.	·1960
level.....		9·50 "	·2520
·1 below surface		9·55 "	·0067
·2 "		9·60 "	·0045
·3 "		9·63 "	·0039
·4 "		9·58 "	·0034
·6 "		9·55 "	·0030
1·0 "		9·62 "	·0025
1·4 "		9·63 "	·0022
2·4 "		9·62 "	·0020
3·65 "		9·59 "	·0019

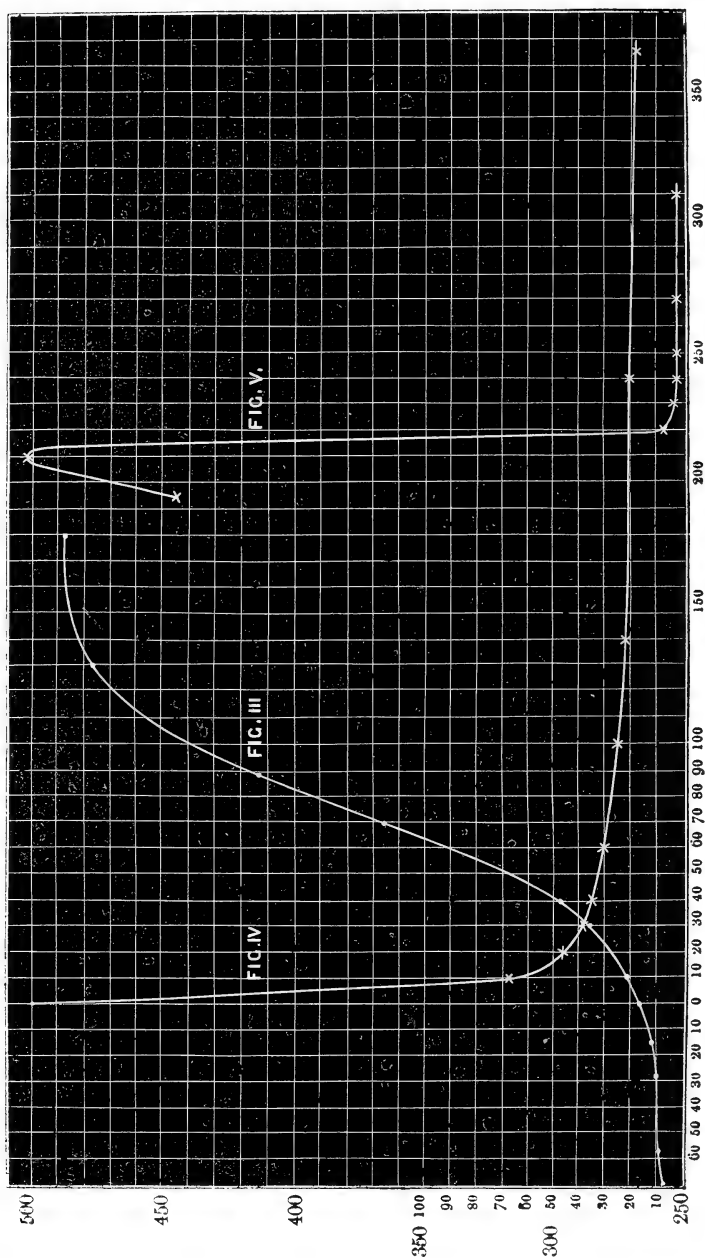
These results are shown on the accompanying curve (p. 412).

With regard to the first two observations, in which the plate was oscillating in the surface of the liquid, only two complete oscillations were obtainable for each determination; and as the logarithmic decrement was found to diminish considerably as the amplitude increased, a number of observations at different amplitudes were taken. These were plotted down in the form of a curve, showing the amplitudes and logarithmic decrement; and from these curves the logarithmic decrement for an arbitrary standard initial amplitude of 500 divisions was taken. The slope of these curves was so considerable that our observations can only be considered as giving an inferior limit to the resistance of the surface of the saponine solution. When the plate was once immersed below the surface, it was found that twenty or more oscillations were readily obtained, and that the magnitude of the original amplitude had little or no effect. The variations of temperature were small (the difference being only 0°·7 C.); and as their effect on the surface-viscosity is unknown, no correction was made for them. The error thus introduced would, however, as the regularity of the curve shows, be small.

These observations, then, enable us to compare the resistance offered to a disk when oscillating in, or just below, the surface of a saponine solution and of water.

Thus we get for the surface of saponine,

$$\frac{M\lambda}{T} = \frac{186653 \times \cdot 252}{9\cdot 5} = 4951;$$



and for the surface of water,

$$\frac{M\lambda}{T} = \frac{753 \times .0266}{5.1} = 3.927.$$

At .1 millim. below the surface these numbers change to 131 and 4 respectively. At the surface, therefore, the ratio of the resistances is 1261; and at .1 millim. below it is 33; while in the interior it is, as has been shown, $\frac{8.42}{6.93}$, or 1.2. Although, therefore, these numbers can only be taken as approximations to the truth, we think that they enable us to make an estimate of the magnitude of the resistance offered to a body oscillating in the surface of saponine solution, for which no previous experiments afforded the required data.

They show that whereas the resistance offered to an oscillating disk, 2 millim. thick, in the surface of water is only about half what it is in the interior, at the surface of a 2-per-cent. saponine solution it is at least 600 times greater than in the interior, but that this ratio is reduced to 16 by immersing the upper surface of the disk to a depth of 0.1 millim.

Special experiments proved that the logarithmic decrement in air was so small that the resistance of the air might safely be neglected when the comparisons of the various resistances were made as above described.

Explanation of the Curves.

Fig. III. is the curve given by the logarithmic decrements obtained from the experiments on water. The abscissæ are expressed in terms of hundredths of a millimetre; they represent the distance of the upper edge of the plate from the surface, and are negative when it is above it. The ordinates represent the logarithmic decrement in terms of .0001, the lowest horizontal line corresponding to the value of .0250.

The gradual increase in the value of the logarithmic decrement as the plate is more deeply immersed is clearly shown.

Fig. IV. refers to the observations made on the saponine solution. In this case the values of the ordinates are to be taken from the small figures. The positions corresponding to the numbers obtained when the disk was in the surface cannot be shown on the scale of the diagram. Fig. V. therefore has been drawn on one tenth of the scale of fig. IV. To avoid confusion it has been displaced to a convenient distance along the line of abscissæ. The enormous increase of resistance as soon as the disk touches the surface is very strikingly shown; and it must be remembered that the increase for a very small oscillation would be very much greater.

The conclusion may be drawn from figs. III. and IV., that both in water and in the saponine solution the effect of the surface disappears when the edge of the disk is about a millimetre and a half below it.

We cannot conclude without expressing our sincere thanks to Prof. Rücker for his kind assistance in our experiments.

LXII. *On Curved Diffraction-gratings.* By R. T. GLAZEBROOK, M.A., F.R.S., Fellow and Lecturer of Trinity College, Demonstrator at the Cavendish Laboratory, Cambridge*.

PROF. ROWLAND has described the appearances presented when a beam of light, after passing through a slit, falls on a grating ruled on a cylindrical surface, and has given a very elegant construction for determining the position of the diffracted foci in the case in which the principal section of the grating is a circle and the source of light is placed at its centre of curvature. The mathematics of the subject have been dealt with still more recently by M. Mascart (*Journal de Physique*, January 1883) and Mr. W. Baily (*Phil. Mag.* March 1883). The object of the present paper is to carry the discussion somewhat further.

Prof. Rowland claims for his gratings that they enable him to form a pure spectrum without the use of lenses, and hence have an immense advantage over those hitherto employed. It must, however, be remembered that the formulæ obtained to give the position of the diffracted spectra are only true to a first approximation, that the spectra formed and the source of light are to one another in the relation of the conjugate geometrical foci of a lens or mirror. All the waves which arrive at any one point of the spectrum are not in exactly the same phase. Aberration effects are produced, and have to be considered just as in the ordinary theory of lenses or mirrors. Now, if a plane wave of light fall on a plane grating, and the effects be observed on a screen at an infinite distance behind the grating, the spectrum formed is perfectly pure; all the red light, after passing the grating, is definitely brought to a focus at one point; there is no aberration, so far at least as the grating is concerned. Of course the difficulty is to obtain the plane wave and the screen at an infinite distance. If the source of light be placed at the principal focus of a collimating lens, the emergent wave differs from a plane by quantities depending on the aberration of the lens; while if the diffracted beam is received on a second lens and a screen

* Communicated by the Physical Society; read April 14, 1883.

be placed in the focal plane of that lens, the screen would practically be at an infinite distance from the grating but for the aberration produced by the lens.

So far, then, as definition merely is concerned, we have to compare the aberration effects produced by these lenses with those caused by the curvature of the grating. Of course a reflexion grating used without lenses has an immense advantage for experiments on the violet or ultra-violet rays which are absorbed by glass.

In considering the aberration, then, we shall follow the method adopted by Lord Rayleigh in his paper on "Investigations in Optics, with special reference to the Spectroscope. Aberration of Lenses and Prisms" (Phil. Mag. January 1880).

Let QA, QP be two adjacent rays diverging from a point Q and falling on the concave side of a circle AP , centre O . Let $QAO = \phi$, $AOP = \omega$, $QA = u$, $OA = a$. Then

$$QAP = \phi + \frac{\pi}{2} - \frac{\omega}{2},$$

$$AP = 2a \sin \frac{\omega}{2}.$$

Hence

$$QP^2 = u^2 + 4a^2 \sin^2 \frac{\omega}{2} - 4au \sin \frac{\omega}{2} \sin \left(\frac{\omega}{2} - \phi \right); \dots (1)$$

and, expanding as far as ω^3 , we find

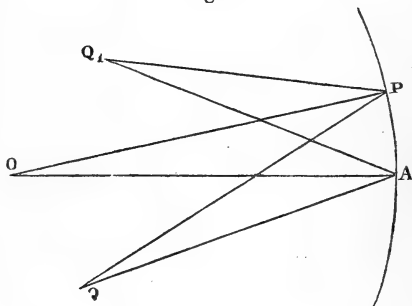
$$\begin{aligned} QP = & u + a\omega \sin \phi - \frac{a\omega^2}{2} \left(\cos \phi - \frac{a}{u} \cos^2 \phi \right) \\ & - \frac{a\omega^3 \sin \phi}{2} \left(\frac{1}{3} - \frac{a}{u} \cos \phi + \frac{a^2}{u^2} \cos^2 \phi \right) + \dots \end{aligned} \quad (2)$$

Again, let Q_1 be another point on the other side of the normal OA , and let $Q_1A = u'$, $Q_1AO = \psi$. Then

$$\begin{aligned} Q_1P = & u' - a\omega \sin \psi - \frac{a\omega^2}{2} \left(\cos \psi - \frac{a}{u'} \cos^2 \psi \right) \\ & + \frac{a\omega^3 \sin \psi}{2} \left(\frac{1}{3} - \frac{a}{u'} \cos \psi + \frac{a^2}{u'^2} \cos^2 \psi \right). \dots \end{aligned} \quad (3)$$

Suppose now that A is a point on one line of the grating, and

Fig. 1.



P a corresponding point on some other line. Then waves from Q diffracted at A and P respectively will reach Q_1 in the same phase if $QP + Q_1P = QA + Q_1A \pm n\lambda$, λ being the wavelength. That is, if

$$\begin{aligned} & a\omega(\sin \phi - \sin \psi) \\ & - \frac{a\omega^2}{2} \left\{ \cos \phi + \cos \psi - a \left(\frac{\cos^2 \phi}{u} + \frac{\cos^2 \psi}{u'} \right) \right\} \\ & - \frac{a\omega^3}{2} \left\{ \sin \phi \left(\frac{1}{3} - \frac{a}{u} \cos \phi + \frac{a^2}{u^2} \cos^2 \phi \right) \right. \\ & \left. - \sin \psi \left(\frac{2}{3} - \frac{a}{u'} \cos \psi + \frac{a^2}{u'^2} \cos^2 \psi \right) \right\} = \pm n\lambda. \quad (4) \end{aligned}$$

This is equivalent to Mr. Baily's formula carried to the next degree of approximation; and his results are obtained by neglecting the term in ω^3 and taking ϕ and ψ to satisfy the equation

$$\sin \phi - \sin \psi = \pm \frac{n\lambda}{a\omega}, \quad . \quad . \quad . \quad (5)$$

and then u and u' to satisfy

$$\cos \phi + \cos \psi - a \left(\frac{\cos^2 \phi}{u} + \frac{\cos^2 \psi}{u'} \right) = 0. \quad . \quad . \quad (6)$$

To consider the aberration we have two cases before us. Let us suppose (1) that equation (5) holds, and determine the value u'_1 , say of u' , considering the terms in ω^3 in equation (4). This will give us what we may call the longitudinal aberration.

In the second case we shall suppose equation (6) to hold, and determine the value for ψ which satisfies (4) to the same approximation. This will give us the lateral aberration.

In the general case equation (4), as it stands, really determines the locus of the image of Q formed by diffraction at the two lines A and P; and this locus is clearly an hyperbola, with A and P as foci. Waves diffracted at A and P respectively will arrive in the same phase at any point of this hyperbola. For every point such as P on the grating an hyperbola possessing similar properties can be drawn. If all these hyperbolas meet in a point, then that point is really a focus for waves diverging from Q; they all are in the same phase when they meet there. This is the case if the grating be plane and Q and Q_1 infinitely distant. If, however, the hyperbolas do not all meet in a point, there is really no focus in its strict sense, only a geometrical focus. If we neglect ω^3 and higher terms, then the point given by (5) and (6) is to this approximation common to all the hyperbolas: it is the geometrical focus.

To discuss, then, the aberration in this case. Let $u'_1 = u' + \delta u'$, where u' satisfies (6), and suppose we neglect $\overline{\delta u'}^2$, $a\omega\delta u'$, and such terms. Then

$$\begin{aligned} \cos \phi + \cos \psi - a \left\{ \frac{\cos^2 \phi}{u} + \frac{\cos^2 \psi}{u'} \left(1 - \frac{\delta u'}{u'} \right) \right\} \\ + a\omega \left\{ \pm \frac{n\lambda}{3a\omega} - \frac{a \sin \phi \cos \phi}{u} \left(1 - \frac{a}{u} \cos \phi \right) \right. \\ \left. + \frac{a \sin \psi \cos \psi}{u'} \left(1 - \frac{a}{u'} \cos \psi \right) \right\} = 0. \end{aligned}$$

Thus

$$\begin{aligned} \delta u' = - \frac{u'^2 \omega}{a \cos^2 \psi} \left\{ \pm \frac{n\lambda}{3a\omega} - \frac{a}{u} \sin \phi \cos \phi \left(1 - \frac{a}{u} \cos \phi \right) \right. \\ \left. + \frac{a}{u'} \sin \psi \cos \psi \left(1 - \frac{a}{u'} \cos \psi \right) \right\}. \quad (7) \end{aligned}$$

Equation (7) determines the aberration in the general case. To determine the effect of this in practice, let us suppose that we are considering the spectrum of the first order, so that the retardation of the light coming from two consecutive lines is just one wave-length; and hence, if P be on the k th line from A, and σ the distance between two lines, then the arc AP = $k\sigma = a\omega$, and $n = k$.

Let us suppose, further, that the origin of light is at the centre of curvature of the grating, so that $u = a$, $\phi = 0$, and hence, taking the -ve sign in (6),

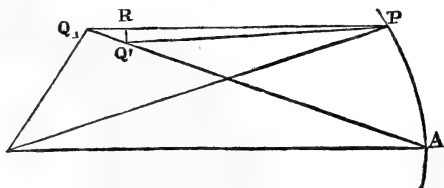
$$\sin \psi = \frac{\lambda}{\sigma}, \cos \psi = \sqrt{\left(1 - \frac{\lambda^2}{\sigma^2} \right)}, u' = a \cos \psi = a \sqrt{\left(1 - \frac{\lambda^2}{\sigma^2} \right)}.$$

Thus

$$\delta u' = + a\omega \frac{k\lambda}{3a\omega} = + \frac{1}{3} k\lambda. \quad (8)$$

Let Q_1 (fig. 2) be the point on the line given by $\sin \psi = \frac{\lambda}{\sigma}$, which is determined by $u' = a \cos \psi$, Q' being the point on

Fig. 2.



that line at which light arrives in exactly the same phase from A and P. Then $Q_1Q' = \frac{1}{3} k\lambda$. And the angle PQ_1A differs by

only a small quantity from ω ; while, since $Q'Q_1$ is small compared with PQ' , the angle Q_1PQ' is small compared with ω . Hence, if $Q'R$ be drawn at right angles to PQ_1 , the light from P arrives at R in the same phase as at Q' , and the difference in phase at Q_1 between the waves coming from A and P is $Q_1Q' - Q_1R = Q_1Q'(1 - \cos \omega)$; and to the same approximation this is equal to $\frac{1}{6}k\lambda\omega^2$. So that if we consider as well the light coming from a point k' lines below A , the extreme difference of phase in the various waves which reach the point Q_1 is $\frac{1}{6}(k+k')\lambda\omega^2$; $k+k'$ will be the total number of lines in the grating.

Thus in one of Prof. Rowland's gratings we have

$$a = 213 \text{ centim.},$$

$$\omega = \frac{37}{2130} \text{ about,}$$

$$k+k' = 14250;$$

and hence the difference in phase is about $7\lambda/10$. Hence the aperture of the grating is too large to give the best definition: for that purpose the difference of phase in the various secondary waves arriving at the point in question should not be greater than $\lambda/4$.

We may conveniently express this difference of phase in terms of the number of lines, the radius of the grating, and the distance between the lines. Let σ be the distance between the lines; then

$$\omega = \frac{(k+k')}{2a} \sigma,$$

and the difference of phase is

$$\frac{1}{24} \times (k+k')^3 \frac{\sigma^2}{a^2} \lambda.$$

For good definition this difference of phase must not be greater than $\lambda/4$. Since in the case above the difference of phase is $7\lambda/10$, we must reduce the number of lines, keeping the distance between them the same, in the ratio of $\sqrt[3]{10}$ to $\sqrt[3]{28}$, or rather more than 2 to 3. Hence by covering up rather less than one third of the grating we should expect to produce better definition.

In another grating of Rowland's, $\sigma = \frac{1}{11400}$ centim., $a = 520$ centim., $k+k' = 160,000$; and in this case the difference of phase comes out to be about $4.8 \times \lambda$. The grating is much too

wide ; it will require reducing in the ratio of $1 : \sqrt[3]{19 \cdot 2}$, or about 3 : 8.

To consider now the lateral aberration, using the same notation, describe a circle (fig. 3) through Q_1 with A as centre. Light from A arrives in the same phase at all points on this circle. Let Q' be the point on the circle at which the light arriving from P is in the same phase as that from A, and let $\psi + \delta\psi$ be the angle OAQ' , and let ψ, ϕ, u , and u' satisfy (5) and (6). Our fundamental equation (4) becomes, neglecting terms like $\omega^2\delta\psi$,

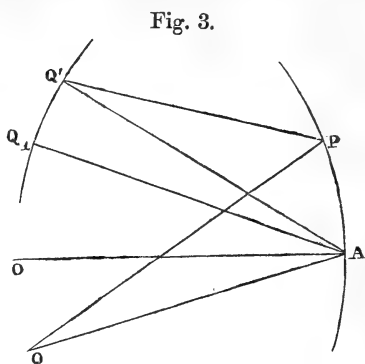


Fig. 3.

$$a\omega \left[\sin \phi - \sin \psi - \delta\psi \cos \psi - \frac{\omega}{2} \left\{ \cos \phi + \cos \psi - a \left(\frac{\cos^2 \phi}{u} + \frac{\cos^2 \psi}{u'} \right) - \sin \psi \delta\psi \left(1 - \frac{2a \cos \psi}{u'} \right) \right\} - \frac{\omega^2}{2} \left\{ \sin \phi \left(\frac{1}{3} - \frac{a}{u} \cos \phi + \frac{a^2}{u^2} \cos^2 \phi \right) - \sin \psi \left(\frac{1}{3} - \frac{a}{u'} \cos \psi + \frac{a^2}{u'^2} \cos^2 \psi \right) \right\} \right] = \pm n\lambda. \quad (9)$$

Hence

$$\delta\psi \left\{ \cos \psi - \frac{\omega}{2} \sin \psi \left(1 - \frac{2a \cos \psi}{u'} \right) \right\} + \frac{\omega^2}{2} \left[\sin \phi \left\{ \frac{1}{3} - \frac{a}{u} \cos \phi \left(1 - \frac{a}{u} \cos \phi \right) \right\} - \sin \psi \left\{ \frac{1}{3} - \frac{a}{u'} \cos \psi \left(1 - \frac{a}{u'} \cos \psi \right) \right\} \right] = 0;$$

and, to the approximation adopted in considering the longitudinal effect,

$$\delta\psi = -\frac{\omega^2}{2} \sec \psi \left\{ \pm \frac{1}{3} \frac{n\lambda}{a\omega} - \frac{a}{u} \sin \phi \cos \phi \left(1 - \frac{a \cos \phi}{u} \right) + \frac{a}{u'} \sin \psi \cos \psi \left(1 - \frac{a}{u'} \cos \psi \right) \right\}. \quad (10)$$

If, as before, Q coincide with O, then $u=0$, $\phi=0$, $u'=a \cos \psi$;

and taking the negative sign, so that $\sin \psi = \frac{n\lambda}{a\omega}$,

$$\delta\psi = \frac{\omega \sec \psi \times n\lambda}{6a}, \quad \dots \dots \dots (11)$$

and

$$Q_1 Q' = u' \delta\psi = \frac{n\lambda \omega}{6}.$$

Thus, carrying our approximation as far as terms in ω^3 in equation (4), we find that the position of the image formed, considering only two of the lines as producing diffraction effects, is not at Q_1 but Q' , where $Q_1 Q' = \frac{n\lambda \omega}{6}$. Hence, if we consider the whole grating, using the same notation as before, the breadth in a direction normal to AQ_1 of the image formed will be comparable with $\frac{(k+k')\lambda\omega}{6}$, ω being the whole semi-aperture. Expressing this in terms of the radius of the grating a and the distance between the lines σ , we find the value $\frac{1}{6}(k+k')^2 \frac{\lambda\sigma}{a}$. Thus the breadth of the image will depend on

the square of the number of lines. In the grating first considered this quantity, $\frac{1}{6}(k+k')\lambda\omega$, is about $\frac{1}{500}$ of a centimetre for yellow light, while the distance between the D lines is about $\frac{1}{50}$ centim., or ten times as much; while in the second grating this lateral aberration is $\frac{1}{50}$ centim., the distance between the D lines being about seven times as great. If the size of this last grating be reduced to $\frac{3}{8}$ of what it actually is, the extreme lateral aberration will be reduced to $\frac{9}{64}$ or about $\frac{1}{7}$ of its actual value, thus becoming about $\frac{1}{350}$ of a centimetre, and the extreme difference of phase in the light of a given wavelength λ reaching any point of the diffracted spectrum will never exceed $\lambda/4$, the dispersion will remain unaltered, the definition and the brightness of the spectrum will both be increased.

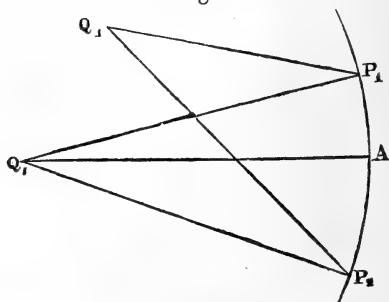
It is clear that in both cases the outer portion of the grating not merely impairs the definition, but actually renders it less bright than before. For

consider two points P_1, P_2 (fig. 4) equidistant from A , such that the difference in phase in the waves coming from P_1 and P_2 to Q_1 is $\frac{\lambda}{2}$ (since the differ-

ence of phase for the extreme rays is in both cases greater than $\frac{\lambda}{2}$, these points

can be found). Then the light reaching Q_1 from above P_1 is

Fig. 4.



opposite in phase to some of that which reaches Q_1 from between P_2 and A, and tends to neutralize the effect of this; while similar results hold for light coming respectively from below P_2 and between A and P_1 . Thus a large aperture does not necessarily mean that there is a large quantity of light at the focus. Exactly the same may happen in the case of a lens. Lord Rayleigh has shown that if α be the angular semi-aperture of the lens as viewed from the focus, and the curvatures of the lens be adjusted to reduce the longitudinal aberration to a minimum, α^4 should not exceed λ/f . A similar course of reasoning shows us that if α^4 is greater than $2\lambda/f$, the light from the outer annulus of the lens will be opposite in phase to that from the central portions.

To compare, finally, the definition of the curved grating with that produced by a plane grating, and two lenses of equal focal length used as a collimator, and the object-glass of a telescope respectively, we can show (Parkinson, 'Optics,' § 130), that if α is the semi angular aperture of either of these lenses seen from its principal focus, f its focal length, and the curvatures are adjusted to make the aberration of each lens a minimum, then the aberration is, for light of refractive index 1.5 , $\frac{15}{7}f\alpha^2$; but, as quoted above, Lord Rayleigh has shown that the aberration should not be greater than λ/α^4 . Hence α^4 must not be greater than $\frac{7}{15} \frac{\lambda}{f}$.

In the case of the first of Prof. Rowland's gratings discussed above, the slit and eyepiece are at a distance of about 200 centim. from the grating. Let us suppose we are using two lenses of 200 centim. focal length, and inquire what their aperture may be to allow the condition above given to be satisfied. If y be the radius of the lens, we have

$$y^4 \text{ not } > \text{ than } \frac{7 \times 8 \times 10^6 \times 6}{15 \times 10^5}.$$

Thus y must not be greater than 3.8 centim. A lens of this aperture would just about admit the light from the whole of the actual grating 5 centim. \times 7 centim. in area if it were plane; whereas, without the lens, to obtain the best definition we are restricted to the use of about two thirds of the grating.

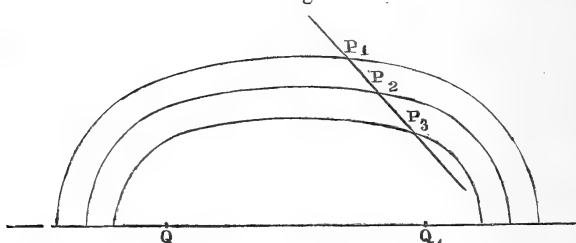
In the case of the other grating, we may, without increasing the size of the apparatus, use a lens of 500 centim. focal length; for good definition its aperture should not be more than

$$\sqrt[4]{\frac{7 \times 125 \times 10^6 \times 6}{15 \times 10^5}},$$

or about 7.6 centim. A lens of this aperture would enable us to use with the best advantage the whole of the grating if it were plane; whereas in the concave grating, for good definition we should only use about three eighths of the whole. It would seem, then, that in cases in which there is no objection to the use of glass (because of its absorbing qualities), a large grating may be used to greater advantage if it be ruled on a flat surface and properly chosen lenses be employed with it, than if the grating be curved.

It may be instructive to consider the subject briefly in another manner. Let Q, Q_1 (fig. 5) be any two points, and

Fig. 5.



with Q and Q_1 as foci describe a series of confocal ellipses; let the major axes of these ellipses increase in arithmetical progression, and let the common difference be λ . Consider a spherical wave diverging from Q and reflected at any point of any one of these ellipses; all the reflected light will reach Q_1 in the same phase. Take any surface P_1, P_2 , &c. cutting the ellipses in P_1, P_2 &c., and suppose it capable of reflecting light at these points and incapable of so doing elsewhere. All the light from Q which falls on this surface at these points will be reflected to Q_1 , and the various waves will reach Q_1 in the same phase. If now Q be the section of a slit normal to the paper, $P_1 P_2$ &c. that of a polished cylindrical surface whose generators are normal to the paper, and lines be ruled on this surface to block out the spaces $P_1 P_2, P_2 P_3$, &c., the lines also being normal to the paper, we shall obtain a diffraction-grating which will give an image of Q without aberration at Q_1 .

We can thus determine the law according to which lines must be ruled on any cylindrical surface to give an aplanatic diffraction-image of a slit; for we require only to write down the equations to the ellipses and the surface and determine the points of intersection. We will solve the simple case when the curve $P_1 P_2$ &c. is a straight line parallel to $Q Q_1$. Take $Q Q_1$ as axis of x ; let a and b be the semi-axes of one of the ellipses, suppose that which touches the line $P_1 P_2 \dots$; then

the semi major axes of the other ellipses are

$$a + \frac{\lambda}{2}, \quad a + \delta \dots a + \frac{n\lambda}{2}, \text{ \&c.};$$

while to find b_n , the semi minor axis of the $(n+1)$ th ellipse, we have

$$\left(a + \frac{n\lambda}{2}\right)^2 - b_n^2 = a^2 - b^2. \text{ A } b_n^2 = b^2 + na\lambda + \frac{n^2\lambda^2}{4}. \quad (12)$$

Let $Q Q_1 = 2c$, then we have

$$a^2 = b^2 + c^2, \quad (13)$$

and the equation to the $(n+1)$ th ellipse is

$$\frac{x^2}{a^2 + na\lambda + \frac{n^2\lambda^2}{4}} + \frac{y^2}{b^2 + na\lambda + \frac{n^2\lambda^2}{4}} = 1. \quad (14)$$

Let x_n be the abscissa of the point in which this is cut by the line $y=b$, then

$$x_n^2 = \frac{\left(a^2 + na\lambda + \frac{n^2\lambda^2}{4}\right)\left(na\lambda + \frac{n^2\lambda^2}{4}\right)}{b^2 + na\lambda + \frac{n^2\lambda^2}{4}}. \quad (15)$$

Substituting for a and giving n the values 0, 1, 2, 3, &c. in order, we can obtain values for x_0, x_1, x_2 &c., and determine thus the position of the lines. A plane grating ruled in this manner would form at Q_1 without aberration an image of Q for light of the given wave-length λ . Of course it would be open to the objection which holds against all such aplanatic arrangements, viz. that they are only good for light of one definite wave-length. If the grating were used for light of a different refrangibility, the image formed would suffer from aberration.

LXIII. *A new Photometer.*
By Sir JOHN CONROY, *Bart., M.A.**

HAVING recently made a considerable number of photometric observations, and learnt by experience the difficulty which attends all such determinations, I venture to bring before the Society the description of a new form of photometer which appears to possess certain advantages over those in use. All such instruments, with the exception of the wedge-photometer, are essentially arrangements for comparing the illuminating-power of two lights, and therefore do not

* Communicated by the Physical Society; read April 28, 1883.

give absolute measures; the one I propose describing is no exception to this general rule.

I had intended to use, in some experiments on the amount of light reflected by metallic surfaces, the ordinary Bunsen's disk; but I found that, owing to the small size of the beam of reflected light, it was impossible to make any satisfactory measurements with the disks in common use, and after trying various photometric arrangements I finally adopted a modification of Ritchie's photometer.

The various forms of shadow-photometers work well; but as the accuracy of the determination depends on the edge of the two shadows coinciding and yet not overlapping, it is necessary to have some arrangement for altering the distance between the screen and the shadow-producer, which adds to the complexity of the apparatus, except indeed when, as in Mr. Harcourt's photometer for gas-work, the variation in the relative intensities of the two lights is caused by the size of one of the flames being altered, and not, as in those arrangements heretofore in use, by altering the distance of the flame from the screen whilst the size is kept constant.

Ritchie's photometer, as is well known, consists of two pieces of white paper fastened to the adjacent sides of a triangular block of wood, each being illuminated by one only of the lights which are to be compared. Finding it impossible so to arrange the apparatus that the illuminated surfaces should be actually in contact, the bend in the paper along the edge of the block separating the two illuminated areas, and therefore interfering with the accuracy of the determination, I placed one of the pieces of paper slightly in front of the other, and overlapping it to a small extent, so that, whilst both were visible to the observer, each was illuminated by one only of the sources of light; when equally illuminated, the edge of the front paper vanished.

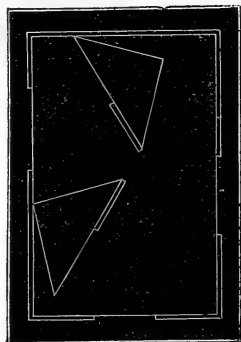
It was originally intended that the light should be incident upon the surfaces of the paper at an angle of 45° ; but it was found that when the light regularly reflected by the paper reached the observer (*i. e.* when the line of sight and the direction of the incident light formed equal angles with the normal to the paper) it was not possible to make satisfactory measurements.

After various positions had been tried, it was found that the best results were obtained when the light was incident upon the paper at an angle of about 30° and the line of sight formed an angle of 60° with the normal.

Two triangular blocks of wood, 4 centim. high, were screwed to a rectangular board about 15 centim. by 10 centim., in the

position shown in the figure, and pieces of white paper, 3 centim. by 3 centim. (filter-paper was tried ; but ordinary white writing-paper not too highly glazed seemed most suitable), held against the hypotenuse of each of the triangular prisms by india-rubber bands.

It is of course essential that the light should be incident upon both papers at equal angles, and that the papers should be so placed that no light can be reflected from one to the other. It is desirable that both papers should be cut from the same sheet, and that the surfaces on which the light is incident should originally have formed one side of that sheet.



$\frac{1}{4}$ actual size.

A rectangular board, similar to that to which the prisms were fixed, was fastened to the top of the prisms by two screws; and to the edges of this board four strips of card, in three of which square apertures had been cut, were fixed, and the whole arrangement painted both externally and internally a dead black.

In order to adjust the papers, or replace them by new ones, it is merely necessary to withdraw the two screws in the top board and lift it off, together with the sides of the box.

The edge of the front paper coinciding with the middle line of the box, the photometer could be used with either side uppermost; and in order to be certain that the illumination of both papers was entirely due to light incident directly upon them, measurements of the relative intensity of two similar paraffin-lamps were made with the photometer in both positions; and it was found that the readings were identical.

The photometer was compared with a Bunsen's disk by placing it at the end of a horizontal board furnished with a scale, and along which a paraffin-lamp was arranged to slide. A Bunsen's disk, in an ordinary form of support with two inclined looking-glasses, could be screwed to the end of the board, to which three stops were so fixed that, when the disk was removed and the new photometer placed against the stops, the middle line of the box was in the same vertical plane as the disk had been.

A paraffin-lamp was placed on either side of the photometer, the position of one remaining constant, whilst that of the other was altered until the illumination was equal, and the distance of the latter read off, in centimetres, on the scale.

The table gives the results of eight observations made with both photometers, the differences of each observation from the mean, and also the squares of these differences.

Bunsen's Disk.

centim.	Differences from the mean.	Squares of the differences.
85.7 + .6	.36
84.6 - .5	.25
84.9 - .2	.04
85.4 + .3	.09
86.2 + 1.1	1.21
84.8 - .3	.09
84.3 - .8	.64
85.2 + .1	.01
Mean . . 85.1		Sum . . 2.69

New Photometer.

85.0 - .4	.16
85.7 + .3	.09
85.0 - .4	.16
85.3 - .1	.01
85.2 - .2	.04
85.4 - .0	.0
85.8 + .4	.16
85.7 + .3	.09
Mean . . 85.4		Sum . . 0.71

The probable error of the mean result and the probable error of a single observation were found by the ordinary formulæ,

$$0.6745 \sqrt{\frac{\text{sum of the squares of the differences}}{n(n-1)}} \text{ and } \sqrt{n} \times \text{the}$$

probable error of the mean result, n being the number of observations.

	Bunsen's disk. centim.	New photometer. centim.
Probable error of mean result	± 0.148	± 0.076
„ „ single observation	± 0.418	± 0.215

The new photometer therefore appears to be twice as accurate as the Bunsen's disk : it is only fair to add that, had the measurements been made by an observer accustomed to work with the disk, the result might have been different.

LXIV. *On a Theory of the Electric Discharge in Gases.* By J. J. THOMSON, M.A., *Fellow of Trinity College, Cambridge**.

THE aim of the following article is to give an account of a theory which seems to explain some of the more prominent phenomena of the electric discharge in gases, and which also indicates the presence in the electric field of stresses consisting of tension along the lines of force combined with pressures at right angles to them. Maxwell, as is well known, showed that stresses of this character would explain the mechanical actions between electrified bodies.

I shall take the vortex-atom theory of gases as the basis of the following remarks, as it possesses for this purpose advantages over the ordinary solid-particle theory; though much of the reasoning will hold whichever theory of gases be assumed. As the vortex-atom theory of gases is not very generally known, I shall begin by quoting the more important consequences of this theory which are required in this article. According to this theory, the atoms of gases consist of approximately circular vortex rings. When two vortex rings of equal strength, with (as we shall suppose for simplicity) their planes approximately parallel to each other and approximately perpendicular to the line joining their centres, are moving in the same direction, and the circumstances are such that the hinder ring overtakes the one in front, then if, when it overtakes it, the shortest distance between the circular axes of the rings be small compared with the radius of either ring, the rings will not separate, the shortest distance between their circular axes will remain approximately constant, and these circular axes will rotate round another circle midway between them, while this circle moves forward with a velocity of translation which is small compared with the linear velocity of the vortex rings round it. We can prove that in this case the product of the momentum and velocity of the rings is greater than the sum of the products of the same quantities for the rings when they were separated by a distance great compared with the radius of either. We may suppose that the union or pairing in this way of two vortex rings of different kinds is what takes place when two elements of which these vortex rings are atoms combine chemically; while, if the vortex rings are of the same kind, this process is what occurs when the atoms combine to form molecules. If two vortex rings paired in the way we have described are subjected to any disturbing influence, such as the action due to other vortex rings in their neighbourhood, their radii will be changed

* Communicated by the Author.

by different amounts; thus their velocities of translation will become different, and they will separate. We are thus led to take the view of chemical combination put forward by Clausius and Williamson, according to which the molecules of a compound gas are supposed not to always consist of the same atoms of the elementary gases, but that these atoms are continually changing partners. In order, however, that the compound gas should be something more than a mechanical mixture of the elementary gases of which it is composed, it is evidently necessary that the mean time during which an atom is paired with another of a different kind, which we shall call the paired time, should be large compared with the time during which it is alone and free from other atoms, which time we shall call the free time. If we suppose that the gas is subjected to any disturbance, then this will have the effect of breaking up the molecules of the compound gas sooner than would otherwise be the case. It will thus diminish the ratio of the paired to the free time; and if the disturbance be great enough, the value of this ratio will be so much reduced that the substance will no longer exhibit the properties of a chemical compound, but those of its constituent elements: we should thus have the phenomenon of dissociation or decomposition.

The pressure of a gas in any direction is, according to the vortex-atom theory of gases, proportional to the mean value of the product of the momentum and velocity in that direction, just as in the ordinary solid-particle theory.

Let us now suppose that we have a quantity of gas in an electric field. We shall suppose, as the most general assumption that we can make, that the electric field consists of a distribution of velocity in the medium whose vortex-motion constitutes the atoms of the gas; the disturbance due to this distribution of velocity will cause the molecules of the gas to break up sooner than they otherwise would do. Thus the ratio of the paired time to the free time will be diminished. Now, when the atoms are paired, the product of the momentum and velocity for the compound molecule is greater than the sum of the products of the same quantities for the constituent atoms when free, but the pressure in any direction is proportional to the mean value of the product of the momentum and velocity in that direction. Thus each atom will contribute more to the pressure when it is paired than when it is free; and thus, if the ratio of the paired to the free time be diminished, the pressure will be diminished. Now, according to any conception which can be formed of the distribution in the medium of the velocity due to the electric field, the variation in the velocity will be greater along the

lines of force than at right angles to them; and thus those molecules which are moving along the lines of force will be split up into atoms sooner than those moving at right angles to them. Thus the ratio of the paired to the free time will be less for those molecules which are moving along the lines of force than for those moving at right angles to them; and therefore the pressure will be less along the lines of force than at right angles to them. Maxwell, in his 'Treatise on Electricity and Magnetism,' has shown that a distribution of stress of this character will account for the mechanical actions between electrified bodies.

To show that it is conceivable that this cause should produce effects sufficiently large to account for electrostatic attractions and repulsions, it may be useful to point out that the electric tension along the lines of force is very small when compared with the atmospheric pressure; for air at the atmospheric pressure the maximum electric tension is only about $\frac{1}{2000}$ of the atmospheric pressure. The theory that electrostatic attractions and repulsions are due to stresses in the gaseous dielectric admits of an experimental test; for it is evident that, according to this theory, the tension along the lines of force cannot exceed the pressure of the gas. Thus, if we have a gas sufficiently rare to support an electric field so intense that the pressure of the gas does not greatly exceed the electric tension along the lines of force when calculated by the ordinary expression, viz. $KH^2 / 8\pi$ (where K is the specific inductive capacity of the gas, and H the electromotive force), then, if the theory of stress in the gas be correct, the tension along the lines of force will soon reach a maximum value, and will not increase with an increase in the electromotive force. Thus the attraction between the two electrodes in this case would reach a maximum, and would not afterwards increase with an increase in the difference of potential between them. I may point out that, for this to happen, the density of the gas would have to be much less than the density for which the electric strength is a minimum, which the researches of Dr. De La Rue and others have shown to be at a pressure about '6 millimetre. For down to this pressure the electric force necessary to produce discharge is, speaking very roughly, proportional to the pressure, but the electric tension is proportional to the square of the electromotive force. Thus, down to the pressure of minimum strength, the ratio of the greatest electric tension to the pressure of the gas diminishes with the pressure; and it would be no use seeking for any effect such as is described above, except at pressures very much less than this. Taking the formula given by Dr. Macfarlane in the Philosophical Magazine for December 1880, for calcu-

lating the electromotive force necessary to produce discharge from the pressure when this is less than that giving the minimum electrical strength, viz. $V = \cdot 67 / \sqrt[3]{p}$ (where V is the difference of potential per centimetre, and p the pressure in millimetres of mercury), I find that at a pressure of about $\cdot 0001$ of a millimetre of mercury the electric tension just before discharge would equal the pressure of the gas; so that it is at pressures comparable with this that the experiment ought to be tried.

Let us now pass on to the case where the intensity of the electric field is so great that the dielectric can no longer insulate, and the electricity is discharged.

It will be instructive to consider for a moment what happens when a compound gas is raised to such a temperature that it is dissociated, or an elementary one until its molecules are split up into atoms. If the gas is at a low temperature, say $0^{\circ}\text{C}.$, when heat is first applied, so far as we can tell the whole of the heat is employed in raising the temperature and increasing the radiation, and no heat is rendered latent; in other words, the alteration in the molecular structure of the gas absorbs no work. This state of things continues until we approach the temperature at which the gas begins to be dissociated; then a large fraction of the heat supplied to the gas is used up in altering the molecular structure, and only a part of it is spent in raising the temperature and increasing the radiation.

If we look on this from the point of view of chemical combination which we took before, we may regard it as showing that, if any energy be supplied to the gas when the ratio of the paired to the free time is so large that the gas exhibits none of the phenomena of dissociation, the consequent diminution in the ratio of the paired to the free time does not absorb any of the energy; but if the ratio of the paired to the free time be so small that the gas exhibits some of the phenomena of dissociation, then a diminution in the ratio of the paired to the free time will absorb a considerable amount of energy. The same statements will apply to an elementary gas, except that in this case the change of structure consists in splitting the molecules up into atoms of the same kind, while in the compound gas they were split up into atoms of different kinds.

Let us now apply these considerations to the case of the electric discharge. The disturbance to which the gas in an electric field is subjected makes the molecules break up sooner into atoms than they otherwise would do, and thus diminishes the ratio of the paired to the free times of the atoms of the gas; as the intensity of the electric field increases, the dis-

turbance in some places may become so violent that in these regions the ratio of the paired to the free times approaches the value it has when the gas is about to be dissociated. At this point any diminution of this ratio consequent upon an increase in the intensity of the field will absorb a large amount of energy; this energy must come from the electric field; and we should thus get the phenomenon of the electric discharge. The disturbance to which the gas is subjected might very well account for the luminosity of the discharge; whilst the heat produced by the recombination of the dissociated gas, which would occur as soon as the disturbance due to the electric field was withdrawn, would account for the heat produced by the discharge. Only a very small amount of gas would have to be decomposed in order to absorb the electrical energy of the field. Taking the values of the electric force necessary to produce discharge given by Dr. A. Macfarlane (*Phil. Mag.* Dec. 1880), we find that if the dielectric be hydrochloric-acid gas and the gaseous layer be a centimetre thick or more, the electric energy per cubic centimetre will be less than 1000 in C.G.S. units; while the amount of energy necessary to decompose 1 cub. centim. of hydrochloric-acid gas is more than 4×10^9 in the same units. Thus only about one four-millionth part of the gas would have to be decomposed in order to exhaust the energy of the electric field. This quantity, though so small, is yet probably much larger than would in reality be required, as the work which absorbs the energy of the electric field is the splitting-up of the molecules of the hydrochloric-acid gas into hydrogen and chlorine atoms. These atoms will combine with each other to form molecules of hydrogen and chlorine respectively, and will give out heat in so doing, which will heat the gas, but will not restore the electric energy. Now the heat of combination of hydrogen and chlorine when they form hydrochloric acid is not the same as the heat required to split up hydrochloric acid into atoms of hydrogen and chlorine, but is equal to the latter quantity minus the heat given out when the atoms of hydrogen and chlorine combine to form molecules of hydrogen and chlorine respectively. The determinations by Prof. E. Wiedemann of the heat given out when hydrogen atoms combine to form molecules, and by Prof. Thomsen of the same quantity for carbon atoms, seem to show that these quantities are greater than the heat given out in ordinary chemical reactions, and thus that these latter quantities are the differences of quantities each much greater than themselves. Thus to decompose hydrochloric acid into hydrogen and chlorine would require much more energy than the mechanical equivalent of the heat of combination of hydrogen and chlorine.

This view of the electric discharge indicates a relation between the electric strength of a gas and its chemical properties; for in order to make the spark pass through an elementary gas we have to decompose the molecules into atoms: thus the stronger the connexion between the atoms in the molecule, the greater the electric strength. Thus, for example, we should expect that the atoms of nitrogen are much more firmly connected together in the molecule than the atoms of hydrogen, as the electric strength of nitrogen is much greater than that of hydrogen. Unfortunately we seem to know very little about the strength of connexion between the atoms in the molecule; it would, however, be interesting to try whether a gas whose molecules, like those of iodine vapour, are easily dissociated into atoms would be electrically weak. In many cases, of course, the decomposition of the gas on the passage of the spark is very evident; a common way of decomposing a gas being to pass sparks through it. We might, however, have chemical decomposition without being able to detect the products of it; for these might recombine as soon as the disturbance produced by the electric field was removed. In the case of an elementary gas, the splitting-up of the molecules into atoms will effect the same purpose as the decomposition of the compound gas—*i. e.* the exhaustion of the electric field. Thus, according to the view we are now discussing, chemical decomposition is not to be considered merely as an accidental attendant on the electrical discharge, but as an essential feature of the discharge, without which it could not occur.

Let us now consider what effect rarefying the gas would have upon its electrical strength. In a rare gas the mean distance between the molecules is greater than in a dense one; and if the temperature be the same in both cases, and consequently the mean velocity of the molecules the same, the ratio of the free to the paired time will be greater for the rare than for the dense gas; for the free atoms will, on an average, be longer in meeting with fresh partners. Thus the rare gas will be nearer the state in which it begins to suffer dissociation than the dense gas, and thus it will not require to be disturbed so violently as the dense gas in order to increase the ratio of the free to the paired time to its dissociation value; and thus the intensity of the field necessary to produce discharge would be less for the rare gas than for the denser one: in other words, the electric strength would diminish with the density; and this we know is the case. It is now generally admitted that rare gases are more easily dissociated than denser ones. In fact Sir C. W. Siemens takes this as the basis of his theory of the Conservation of Solar Radiation, as he

supposes that the rays of the sun are able to dissociate the compound gases, chiefly hydrocarbons, which in a very rare state he supposes distributed throughout the universe; while, when these gases exist at pressures comparable with that of the atmosphere, they are able to transmit the sun's rays without suffering dissociation. These considerations would seem at first sight to indicate that the electric strength of gases would continually decrease with the density; whereas we know that it only does so to a certain point, and that afterwards the electric strength increases as the density decreases.

We have in the above reasoning, however, supposed that whenever we got chemical decomposition at all we had always sufficient energy absorbed to exhaust the electric field. In consequence of the great absorption of energy in chemical decomposition, this is legitimate, unless the gas be very rare; but for a very rare gas it will be necessary to decompose a larger proportion of the molecules of the gas, and it will require a more intense electric field to do this. If the gas were very rare, it might be that the energy required to decompose all the gas was not sufficient to exhaust the energy of the electric field. In this case all the electricity could not be discharged at once; while in an absolute vacuum there would be no chemical decomposition to lessen the energy of the electric field, and there would be no electrical discharge at all. Thus there are two causes at work which produce opposite effects on the electric strength as we rarefy a gas. The first is that the gas is more easily dissociated as we rarefy it; this diminishes the electric strength of the gas. The second is that, as there are fewer molecules, a larger proportion of them must be decomposed in order to exhaust the same amount of energy, and it will require a more intense electric field to separate the larger proportion; this will tend to increase the electric strength of a gas as we rarefy it. The second of these considerations is not important at pressures comparable with that of the atmosphere, as in this case the percentage of the gas which has to be dissociated in order to exhaust the energy of the electric field is extremely small; so that, starting from the atmospheric pressure, we should expect the gas to get electrically weaker as it gets rarer. With very rare gases, on the other hand, the second consideration, as the extreme case of a perfect vacuum shows, is the more important; and thus with very rare gases the electric strength should increase as the gas gets rarer. Both of these results agree with the results of experience.

It may be worth while to point out that, according to the view taken in this paper, a perfect vacuum possesses infinite electric strength; and thus it is in opposition to the theories

put forward by Prof. Edlund and Dr. Goldstein, in both of which a vacuum is regarded as a perfect conductor.

In a future paper I hope to explain other phenomena of the electric discharge by means of this theory, and also to apply it to the case of conduction through metals.

LXV. *A new Form of Horse-power Indicator.*

By FREDERICK JOHN SMITH, B.A.*

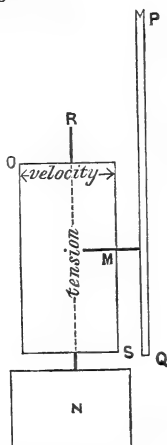
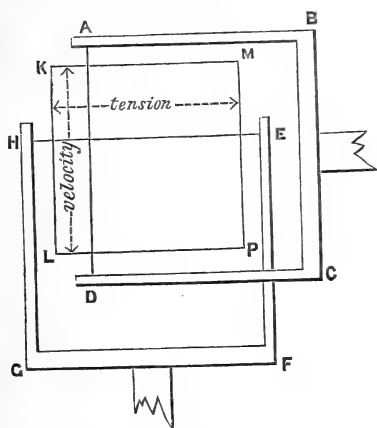
IN transmission-dynamometers and other instruments used to measure, say, engine-power, it is very convenient (in addition to taking the final result by an integrating apparatus, such as that shown at page 87, *Phil. Mag.* Feb. 1883, by the author of these lines) to be able to see on a dial at a glance without any calculation the rate at which at any instant energy in foot-pounds is being transmitted. In the communication just cited it was shown how the velocity at any instant could be determined. Now, supposing that the velocity could be kept uniform, then the tension of the belt, as shown by the dynamometer spring, would only have to be read and multiplied by the velocity of the belt in feet per second; and thus after a trial lasting, it might be, for two minutes the horse-power could be determined. The object of the instrument now to be described is to do away with making different observations and calculations, and to enable the observer to see at a glance the rate at which horse-power is being transmitted at any instant. The instrument in its most simple form is shown at fig. 1, where K P is a square dial divided into little squares (not shown); in each little square the product of the numbers at the top and side appear. A B C D and E F G H are two frames carrying the wires A D and H E. K L is the line of velocities; K M the line of tensions. Now, by means of a centrifugal speed-indicator, such as that of Young, and the introduction of certain mechanism, the wire H E is caused, by its movement across the dial, to indicate the velocity, in feet per second, at any instant; at the same time A D indicates the tension of the belt transmitting the energy, in pounds; and at the point of intersection of the two wires we find these two quantities multiplied together; and thus at a glance we can see what is going on in the dynamometer. *E. g.* suppose the wire H E cuts 80, it then indicates the rate of 80 feet per second; if at the same moment A D cuts 40, it shows that the tension of the belt is 40 pounds; if, then, we look at the point of intersection of the two wires, we find 3200, *i. e.* the rate of 3200 foot-pounds per second at the instant when the observation was made. Fig. 2 shows another form of the same

* Communicated by the Author.

instrument; in it we have the dial in a cylindrical shape. This cylindrical dial is mounted on a speed-indicator N; and the

Fig. 1.

Fig. 2.



pointer M is controlled by the tension spring-rod P Q; and thus M points directly to the figures indicating the rate at which energy, in foot-pounds per second, is being transmitted. In order that the speed-indicators may be calibrated with great accuracy, they are attached to wheelwork driven by a water-wheel of large power in comparison with that required to drive the indicator; if the water-wheel be well balanced and fed by a constant head, very good results can be obtained. The velocity of the water-wheel is measured by means of a counter read after short periods of time.

Taunton, May 18, 1883.

LXVI. On *Polarizing Prisms*.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

IN the very able paper of Mr. R. T. Glazebrook, F.R.S., which appears in your May issue, a polarizing prism is described which, according to the description, "differs from the one described by Professor S. P. Thompson (*Phil. Mag.* Nov. 1881) *only in the fact that its ends are normal to its length* instead of being inclined obliquely to it."

I was not aware that in my paper "On a New Polarizing Prism," which you did me the honour to print in the *Philosophical Magazine* for November 1881, I had anywhere limited myself to any one special angle for terminal faces to the prism. I did discuss in general terms what the result

would be if the terminal planes were inclined at angles the same as those of a Nicol prism, or if more oblique. And, as a matter of fact, I have had prisms constructed with faces both more oblique and less oblique than those of the ordinary Nicol, and some also with end-faces normal to the axis as in the flat-ended Nicol prisms. My suggestion, of which the main point was the orientation of the reflecting film in a principal plane of section of the crystal, included the particular case which has claimed Mr. Glazebrook's attention. And in the concluding paragraph of my paper of 1881 I claimed, for the construction which I suggested, one of the two further advantages which Mr. Glazebrook has so skilfully worked out, viz. "the advantage of producing a field in which the rectilinear polarization approximates more uniformly and symmetrically to a polarization in one plane than is the case in the ordinary Nicol." As this was a point to which Mr. Glazebrook took exception on the occasion when my paper was read to the British Association, I am the more gratified to find his elegant analytical demonstration of this very feature.

I have only to add that, thanks to a small grant from the Wollaston fund of the Royal Society, I have been able to prosecute some further investigations on polarizing prisms, which will, I trust, be shortly ready for publication.

I am, Gentlemen,

University College, Bristol,
May 19, 1883.

Your obedient servant,
SILVANUS P. THOMPSON.

LXVII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 368.]

April 25, 1883. J. W. Hulke, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Skull of *Megalosaurus*." By Prof. R. Owen, C.B., F.R.S., F.G.S.

2. "Notes on the Bagshot Sands." By H. W. Monckton, Esq., F.G.S.

The author explained that his paper related to the series of Bagshot Sands on and around Bagshot Heath, which forms what is termed "the main mass" of the Bagshot beds in the memoirs of the Geological Survey.

The railway-cutting at Goldsworthy Hill, described in 1847 by Prof. Prestwich, is still the best type section of the Middle Bagshot Sands; and the succession of strata seen there was illustrated by reference to newer sections near Ascot and Wellington College. It was pointed out that the most marked feature in this part of the series is very pure greensand, containing casts of shells of Bracklesham species, that a pebble-bed is found at nearly the same relative level over a large area, and that this pebble-bed forms the most

convenient and natural line of division between the Upper and Middle Bagshot.

The Upper Bagshot Sands were then described, and attention was drawn to the abundance of fossils in some of the higher beds.

The author then referred to the correlation of the Bagshot beds with the Hampshire series, and stated his conclusion that the Middle Bagshot beds are of Middle Bracklesham age, whilst the Upper Bagshot Sands are nearly equivalent to the Lower Barton of Hampshire, and are in no way equivalent to the so-called Upper Bagshot Sands of Long Mead End.

3. "Additional Note on Boulders of Hornblende Picrite near the western coast of Anglesey." By Prof. T. G. Bonney, M.A., F.R.S., Sec. G.S.

In the 37th volume of the 'Quarterly Journal' the author described a large boulder of Hornblende picrite which he had discovered near Pen-y-carnisiog. During an excursion last summer to Porth-nobla he had found in that neighbourhood at least eight more. The rock then clearly was not very rare in this part of Anglesey. These exhibited some varietal differences. The author gave some details of their microscopic structure, and an analysis of one identical with the Pen-y-carnisiog rock, kindly made for him by Mr. J. A. Phillips, F.R.S., from which it appeared that this British picrite corresponds fairly well with the picrite of Schriesheim in the Odenwald, to which in other respects it bears so close a resemblance. Mr. J. J. H. Teall recently called the author's attention to a rock which he had collected on Little Knott, east of Bassenthwaite, which appeared to him to resemble the description of the Anglesey picrite. The author had examined a series of specimens from that locality, and found that macroscopically and microscopically there was a marked resemblance, while the percentages of silica and magnesia were not very different. He thought, then, it was very probable that the Anglesey boulders came from the Little-Knott district.

LXVIII. *Intelligence and Miscellaneous Articles.*

MICA FILMS FOR POLARIZING-PURPOSES.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IT may be worth while to mention, in connexion with Mr. Wright's paper on Crystalline Films published in the *Philosophical Magazine* for this month, that very good, clear, uniform mica films may be found among the thin sheets of mica used by photographers for protecting prints, and sold under the name of "crystal medium."

The very best and clearest mica (chiefly, I believe, from Italy and America) is sought for by those who prepare these sheets; and the accuracy and uniformity of the splitting is very great.

Out of a dozen films, 10×6 centim. (*carte-de-visite* size), I have found at least four which are approximately $\frac{1}{4}$ -wave films, uniform in thickness throughout; and most of the others were so even and clear that they were worth the trouble of splitting into thinner ones.

Phil. Mag. S. 5. Vol. 15. No. 96. June 1883. 2 I

For splitting the films I know of no better plan than the old device for splitting a sheet of paper in two, viz. to glue the mica between two pieces of fine cambric and (when they are dry) tear them asunder; a film remains attached to each, and can be separated by immersing the whole in hot water.

It is easy in this way to get films so thin that they only give two "Talbot's bands" between the Fraunhofer lines D and F, when the light reflected from them at an angle of 45° is examined in the spectroscope; and I have obtained many (but not very large) films thinner than this, showing brilliant colours of Newton's first order. Such films are, of course, very fragile; but they can with care be floated upon a glass plate immersed in the water, and then lifted out and dried.

H. G. MADAN.

Eton College, May 15, 1883.

TO CUT A MILLIMETRE-SCREW. BY CHARLES K. WEAD.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

The simple method that Mr. Bosanquet has used in his "Arrangement for dividing Inch- and Metre-Scales," described in the March number of this Magazine, seems to me of so much value that I send you a brief description (communicated to Silliman's Journal, March 1882) of another application of the same principle, *i. e.* of the use of a wheel of 127 teeth or divisions.

In this connexion two points may be worthy of record here:—First, that Rogers's measurements, reported at the Montreal Meeting of the American Association, and confirmed in a general way by E. S. Pierce, make the metre = 39.37015 inches; this is longer than what I have called the "mechanical" metre ($= \frac{10000}{2 \times 127}$ inches)

by only 1 part in 560,000. Secondly, the absolute error of some fine screws is many hundred times this theoretical error: the screw of my dividing-engine is too short by 1 part in 700; of a screw from Perreux at Johns Hopkins University, Rowland states that the "thread is $\frac{1}{2}$ millim., and the head has 250 divisions; 501 divisions give 1 millim. almost exactly." The screw of the engine on which Rutherford's well-known gratings were cut furnishes 17,296 lines to the inch instead of 17,280 ($= 48 \times 360$); so the error here is about 1 part in 1100. In all these cases, and in some others that I have noticed, the screw is too fine. From the details given about the Rutherford screw in the article "Spectrum" in 'The American Cyclopaedia,' it would seem that the shortening here was due to the hardening of the taps and dies used in making the screw. In the mechanical journals methods have been described for giving to a lathe feed-screw a slow longitudinal motion to correct for any error of pitch; but such methods could hardly prove useful on physical apparatus.

I am, yours respectfully,

Ann Arbor, Mich.,
April 14, 1883.

C. K. WEAD.

The increasing attention paid to the metric system renders it

more and more desirable to put the relations between this and the English system of weights and measures in as simple a light as possible. If a screw is to be cut on an ordinary lathe with its pitch a convenient number of teeth to the millimetre, centimetre, or metre, the change-wheels are not adapted to the purpose. The only reference I have ever found to the subject is in Chambers's 'Encyclopædia,' article "Wheelwork," where the problem is given to compute the gears needed to cut a screw of 200 threads to 1 metre on a lathe whose feed-screw has 4 threads to the inch. By continued fractions the number of teeth, beginning with the spindle, is found to be 50, 48, 89, 73; *i. e.*, if x = number of threads to 1 inch, $x = \frac{50 \times 89}{48 \times 73} \times 4 = 5.08$, or in 1 metre are $5.08 \times 39.37 = 200$ threads. This requires two gears with a prime number of teeth.

A simpler method than this I suggested four years ago to Messrs. Buff and Berger, of Boston; and by it they cut me some very satisfactory screws with 1 thread to the millimetre, using their new lathe with two dead centres. As it was new to them and to all my friends familiar with mechanical work to whom I have spoken of it, I venture to publish it, though it seems too simple to have escaped previous notice.

The method is based on the fact that 1 inch equals very nearly 25.4 millimetres; it would equal it exactly

If 1 metre equalled . 39.37008 inches.

Clarke's value equals 39.37043 „

Kater's „ „ 39.37079 „

This "mechanical" metre differs from the best determination yet made (Clarke's) by no more than that differs from the next best one, *viz.* one 110,000th part, though in the opposite direction. The form in which the direction was given to Buff and Berger was:— "Gear the lathe so as to cut a screw with 20 threads to the inch with a gear of 100 teeth on the feed-screw; replace the 100 by

127 ($= \frac{254}{2}$), and the pitch will be 1 millimetre, the theoretical error being much less than the mechanical error of cutting. Thus,

on the lathe above referred to, $x = \frac{127}{20} \times 4 = 25.4$ to the inch, or

1 to the millimetre; again, $x = \frac{127}{100} \times 4 \div 25.4 = \frac{1}{5}$, *i. e.* 1 thread to 5 millimetres.

On small lathes it may often be necessary to put the 127 gear on the arm (because the screw and spindle are too near together), and perhaps to get a longer arm than usual; but neither this nor the calculation for any whole number of threads to the millimetre, centimetre, or decimetre will present any great difficulty to any one familiar with the principle.

Physical Laboratory, University of Michigan,
December 15, 1881.

ON THE CONDENSATION OF FLUIDS ON SOLID BODIES.

BY EILHARD WIEDEMANN.

Wilhelmy*, from experiments on capillarity, concluded that upon glass surfaces which dip into water the latter is condensed in large quantity. Later experiments, however, by Röntgen†, Schleiermacher‡, and Volkmann§, who ascertained the specific gravity of the same body first in whole pieces and then in a state of fine division, showed that the weight of the condensed layer is extremely slight, and not determinable. The latter result is also in perfect accordance with what the newer investigations of Van der Waals|| on the magnitude of the molecular forces have taught us, as I intend, in what follows, to show.

The condensation of the liquid upon the side is owing to this—that the forces emanating from the side, and rapidly diminishing with the distance from it, compress the fluid within a layer the thickness a of which is equal to the sphere of action of the molecular forces. We can conceive the layer in question resolved into thinner ones, each of the thickness dx , within which the molecular force κ can be regarded as constant. If we further assume that the coefficient α of compressibility is independent of the pressure, if s is the specific gravity of the fluid, upon the unit of surface a quantity

$$d = \int_a^0 \kappa \alpha s dx$$

is condensed.

If we put the force κ exerted by the side upon the fluid equal to the pull K exerted by a fluid, in consequence of the molecular forces, upon its superficial parts, we assume a force at any rate of the same order of magnitude as that which acts between the fluid and the side at the surface of contact. But if in the above expression we substitute K for κ , we obtain much too high a value for the amount d condensed, since κ diminishes very quickly with increasing distance from the wall. Consequently we have

$$d < K \alpha s a.$$

For water, $K=10000$ atmospheres, $a=\text{about } 5 \times 10^{-5}$, and, according to Quincke¶, $\alpha=0.0000005$ centim.; so that $d < 10000 \times 0.000005 \cdot 0.0000005 \text{ g.} = 25 \times 10^{-8} \text{ g.}$ Accordingly even $K \alpha s a$, and therefore more certainly d , is a completely inappreciable quantity.

Volkmann's** value for the thickness of the bounding layer, on the assumption of which the laws of the heights to which fluids ascend in capillary tubes and on solid plane sides are valid, cannot be applied to these reasonings, because the real existence of so thick a boundary layer different in constitution from that of the free fluid has not actually been verified.—Wiedemann's *Annalen*, vol. xvii. pp. 988–990 (1882).

* Pogg. *Ann.* cxix. p. 117 (1863), cxxii. p. 1 (1864).

† Wied. *Ann.* iii. p. 321 (1878).

‡ Wied. *Ann.* viii. p. 52 (1879).

§ Wied. *Ann.* xi. p. 182 (1880).

|| 'The Continuity of the Gaseous and Liquid States,' German translation by F. Roth.

¶ Pogg. *Ann.* cxxxvii. p. 402 (1869).

** Wied. *Ann.* xi. p. 117 (1880).

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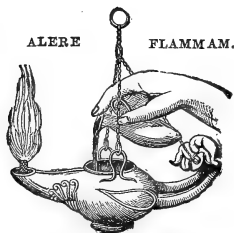


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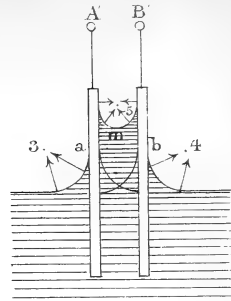
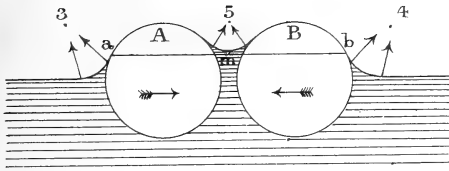


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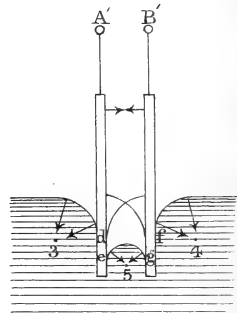
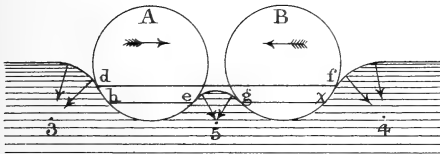


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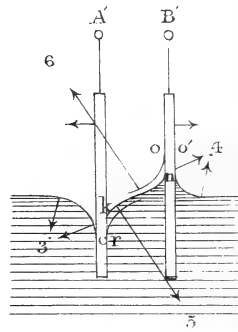
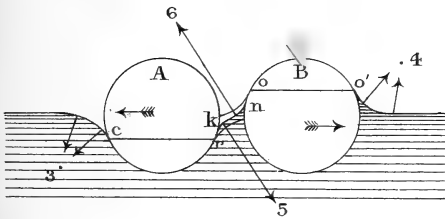
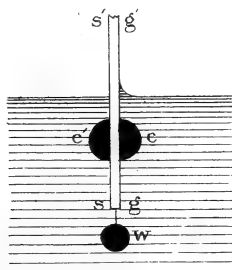


Fig. 4.





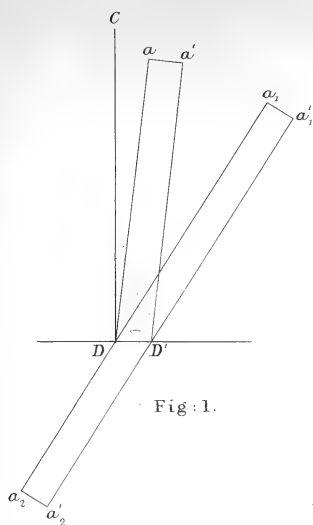


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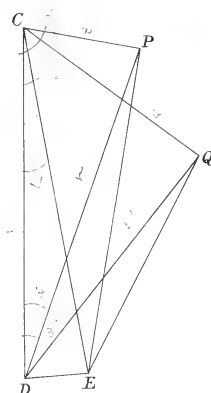


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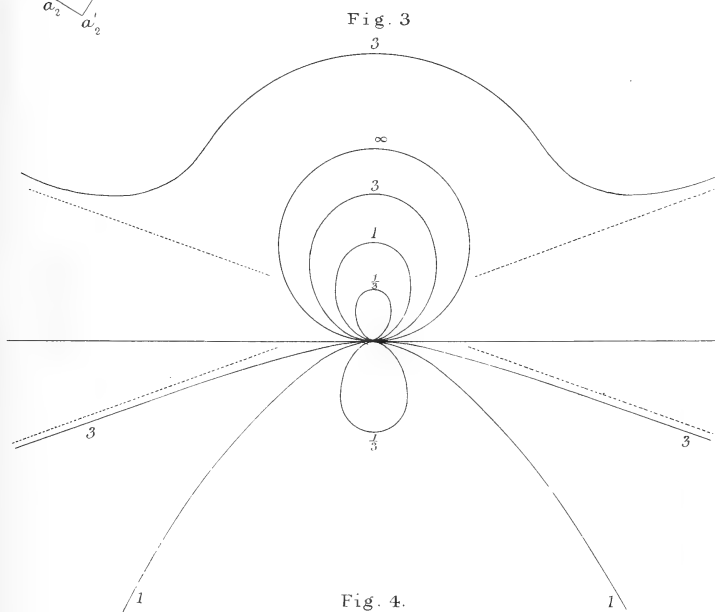
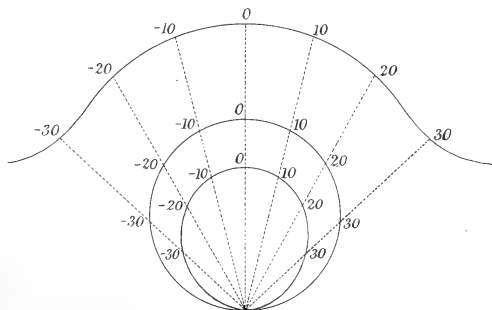
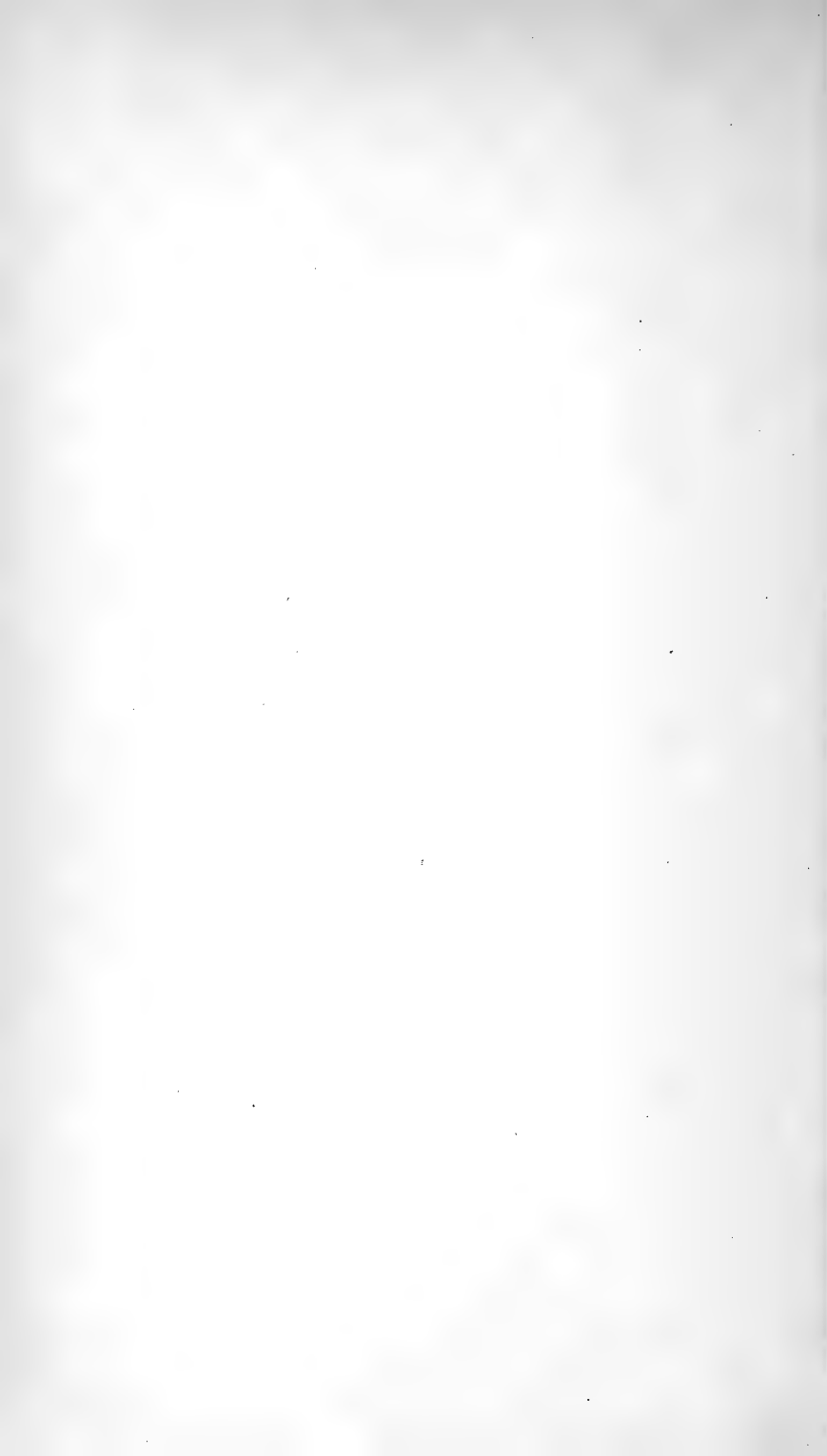
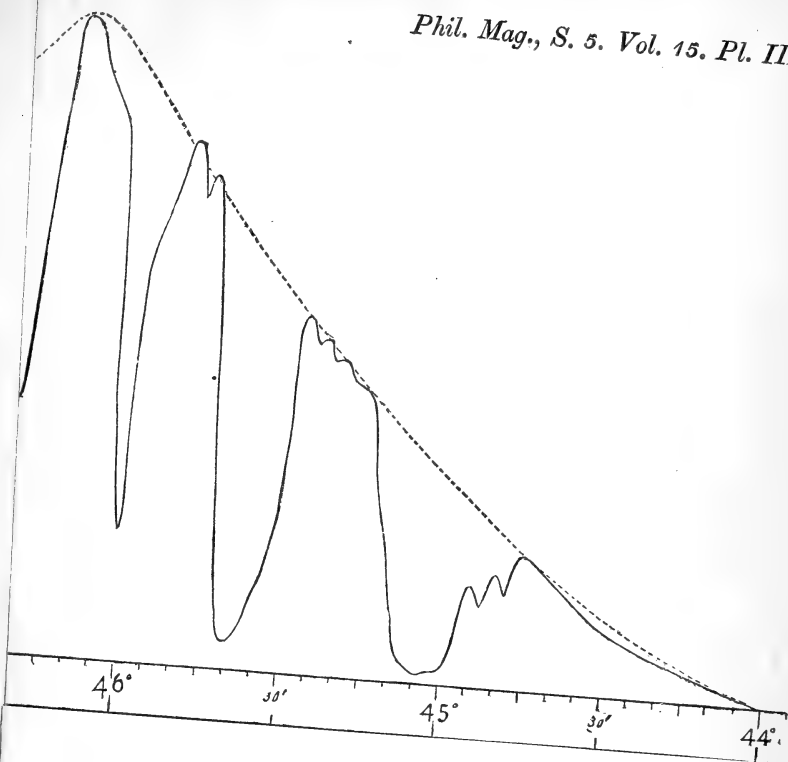


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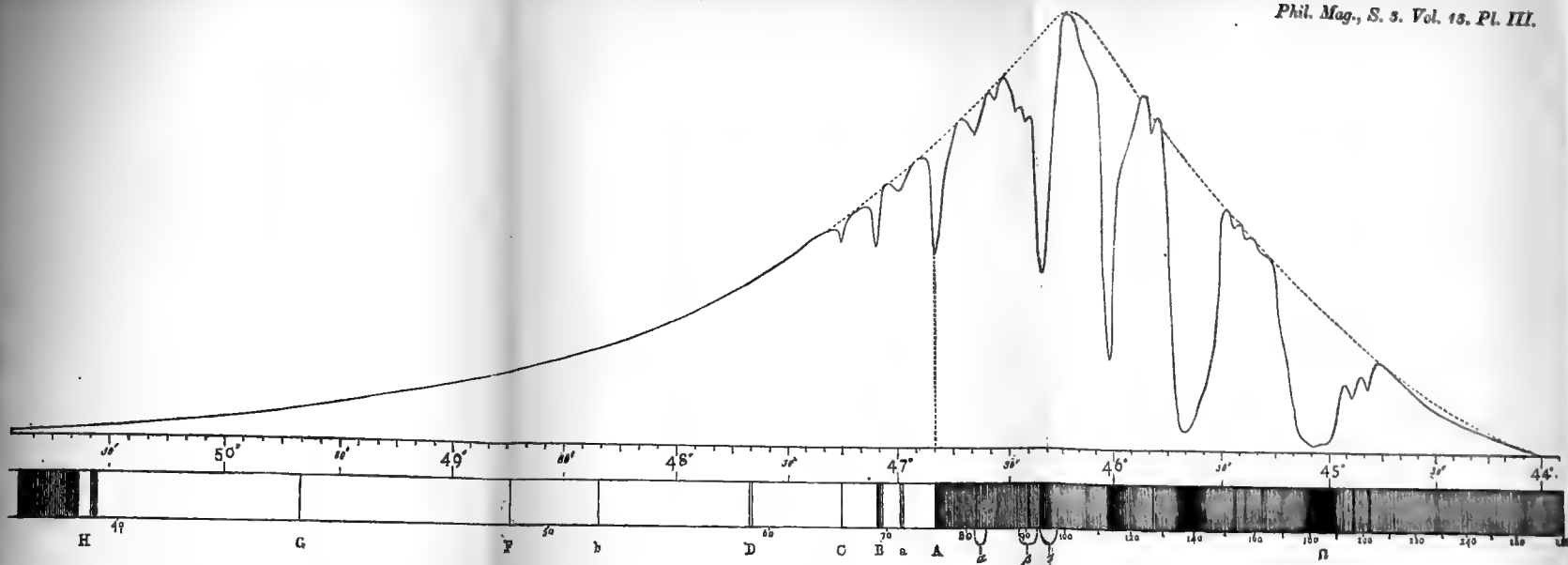




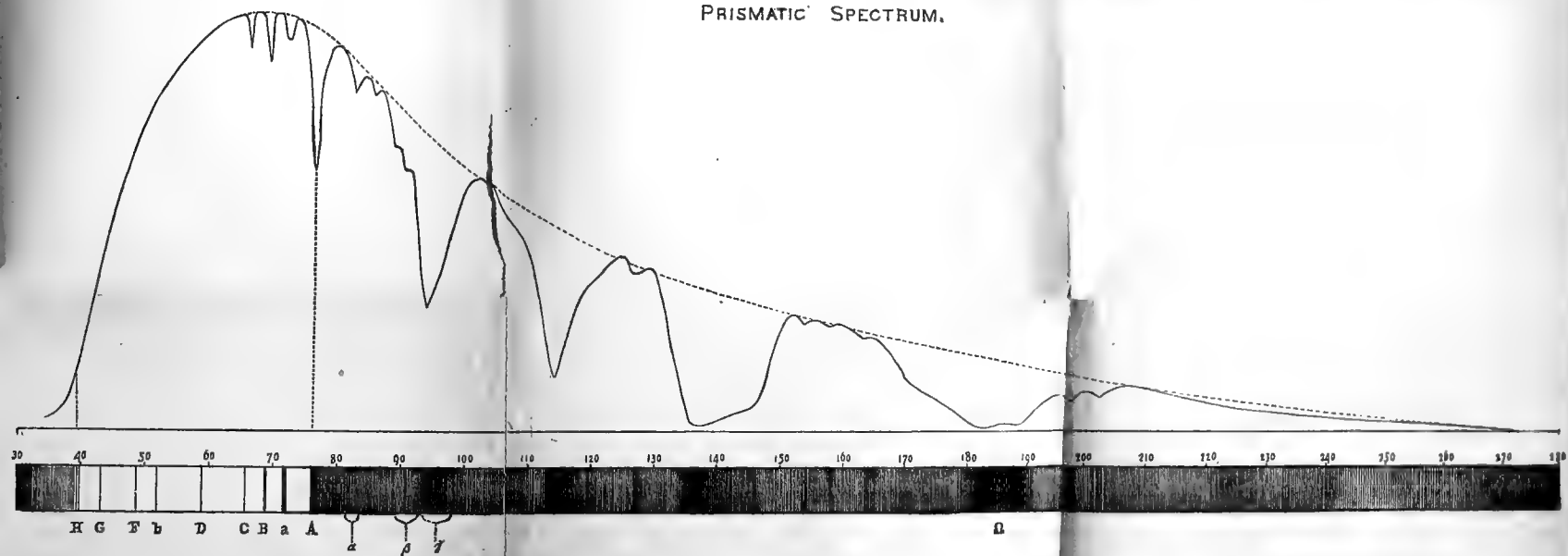
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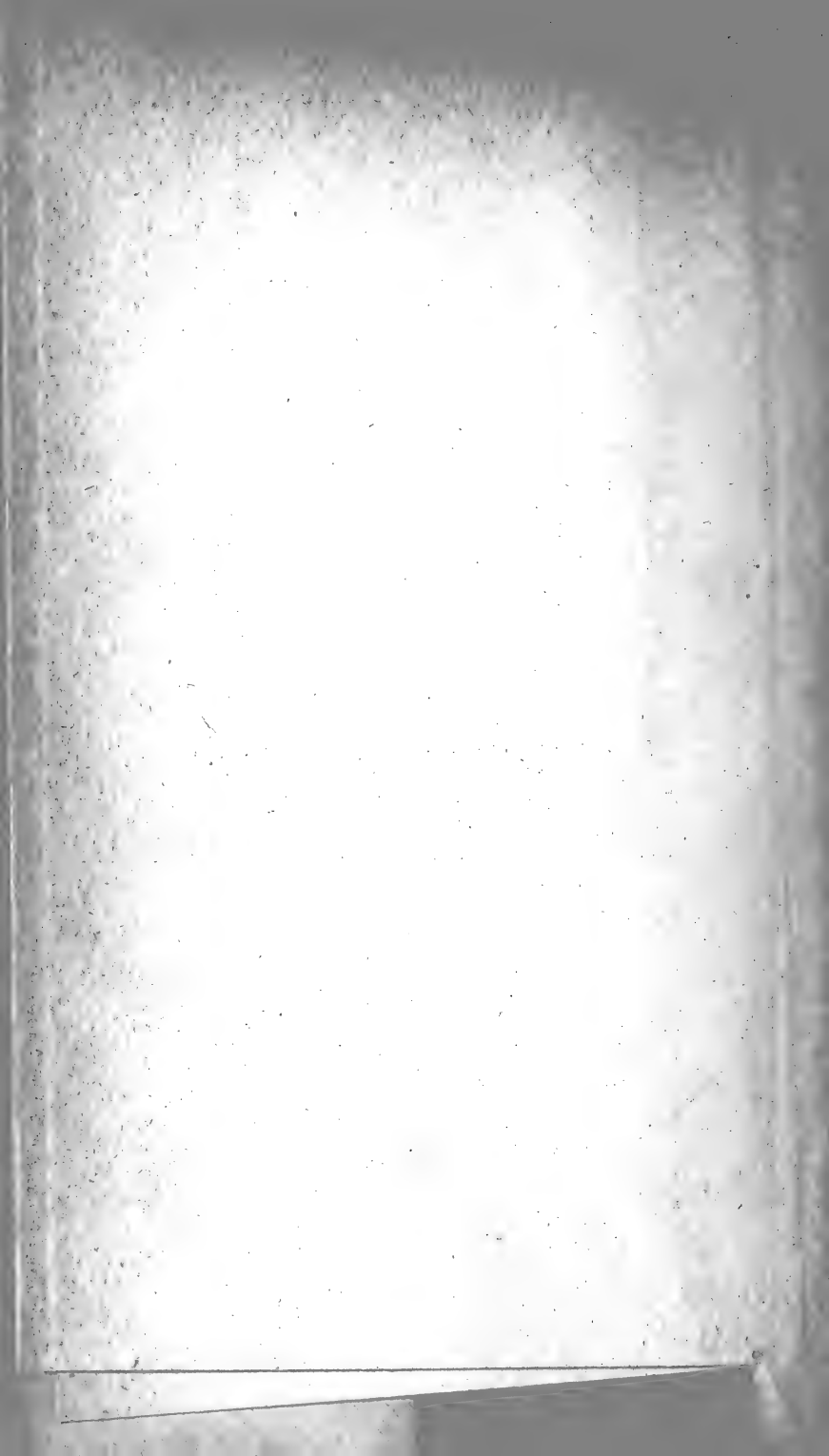




PRISMATIC SPECTRUM.



NORMAL SPECTRUM.



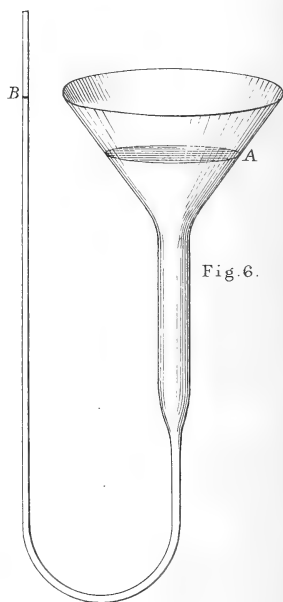
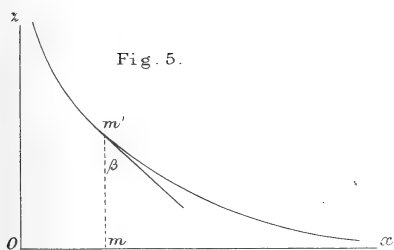
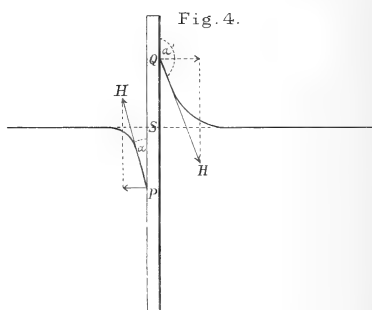
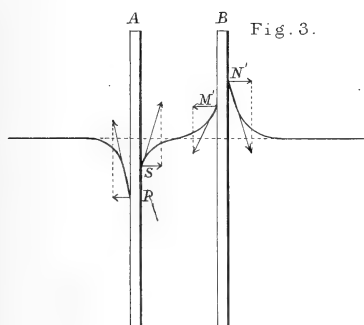
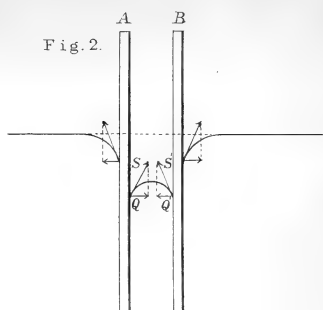
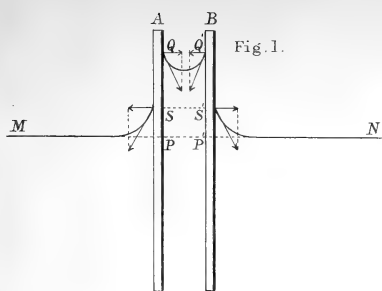




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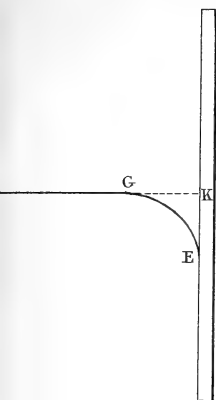


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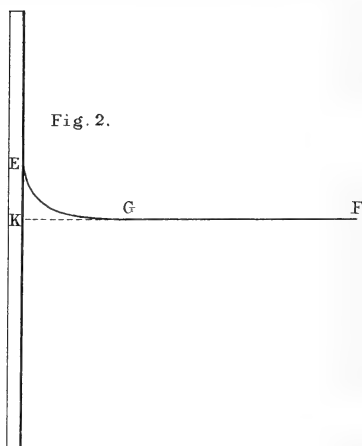


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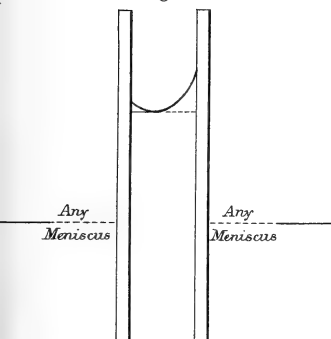


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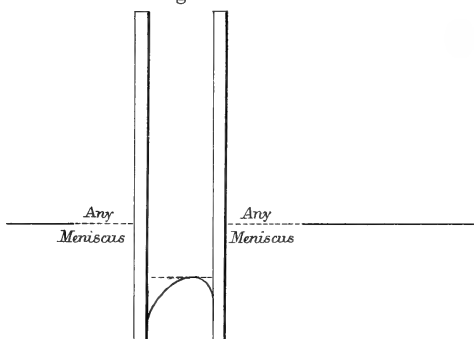


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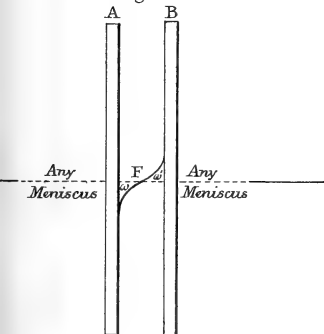
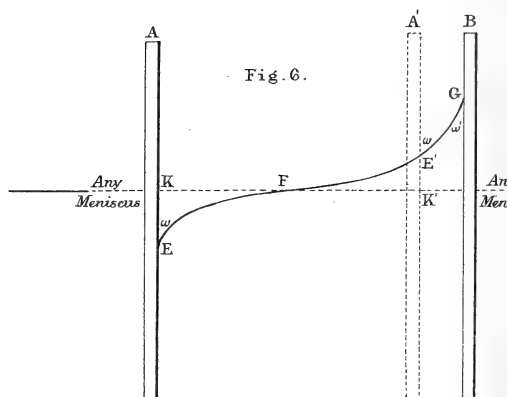


Fig. 6.



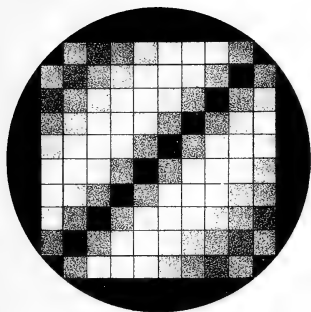


Fig. 1.

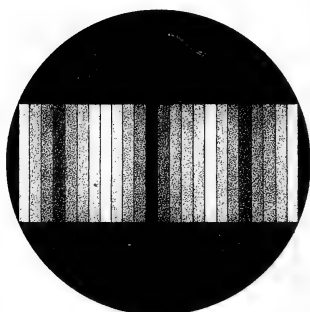
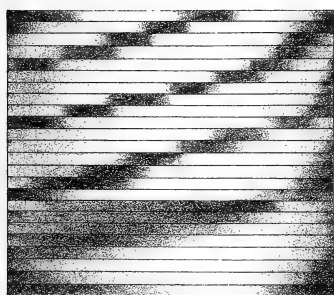


Fig. 2.



Blue end

Fig. 3.

Red end

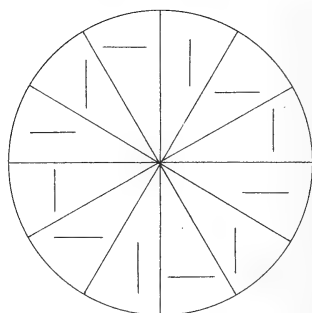


Fig. 4.

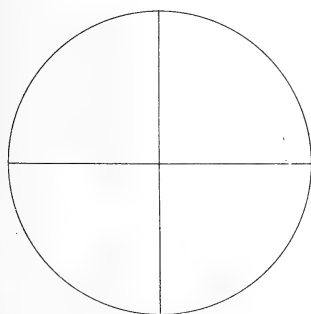


Fig. 5.

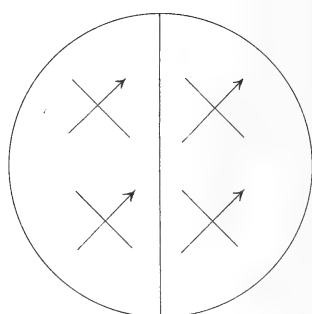


Fig. 6.



Fig

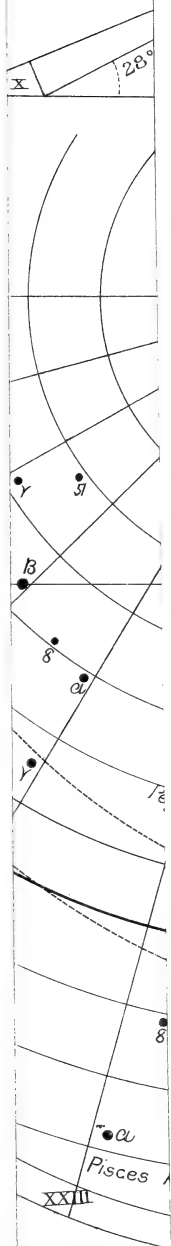


Fig. 1.

1 inch = 100 miles.

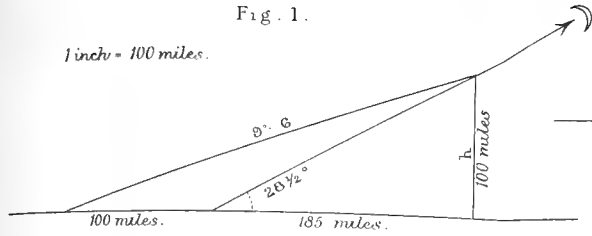


Fig. 2.

1 inch = 100 miles.

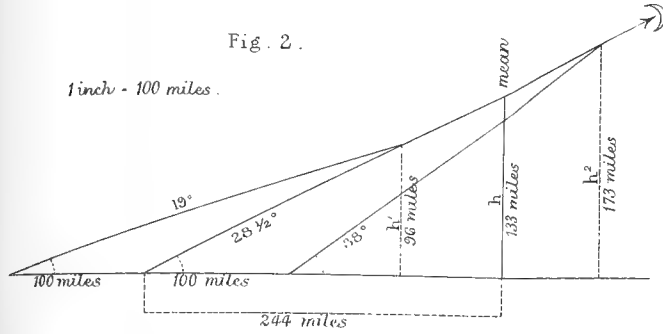


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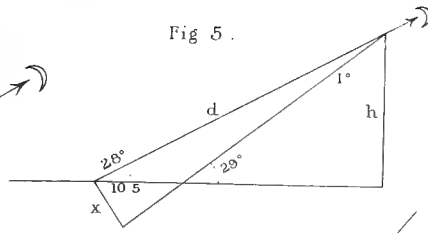


Fig 4.

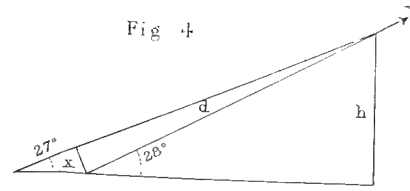


Fig. 3.

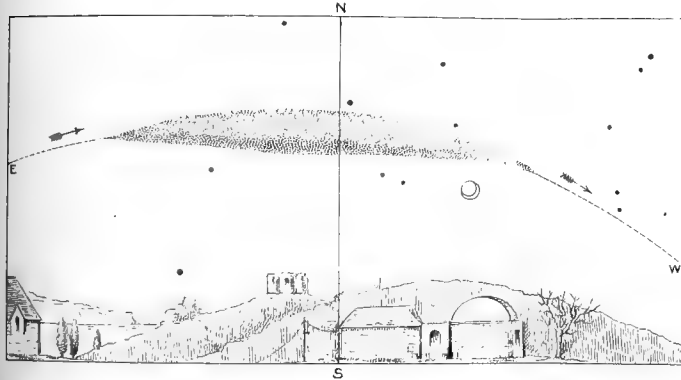
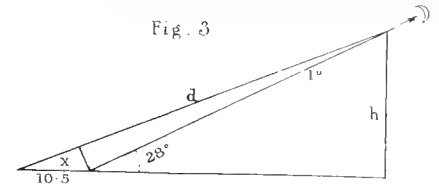


Fig. 6.

Passage of Auroral beam, 17th Nov^r 1882, as seen from Guildown Observatory Lat. 51° 13' 39" N Long 0° 28' 47" W.

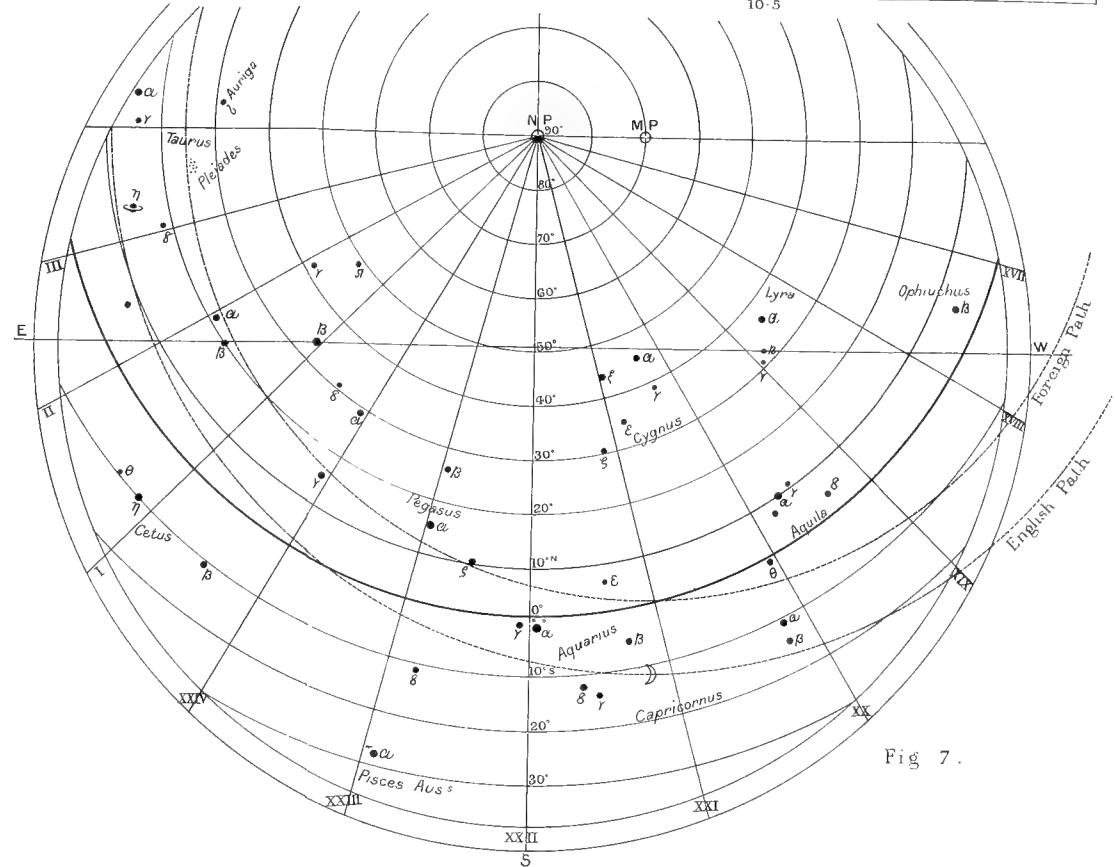


Fig 7.

Course of Auroral beam 17th Nov^r 1882, among the Stars.

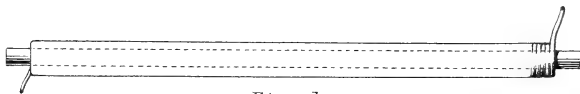


Fig. 1.

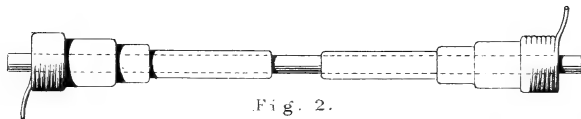


Fig. 2.



Fig. 3.

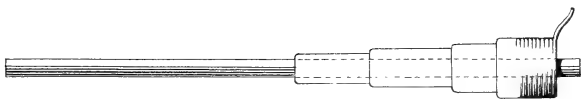


Fig. 4.

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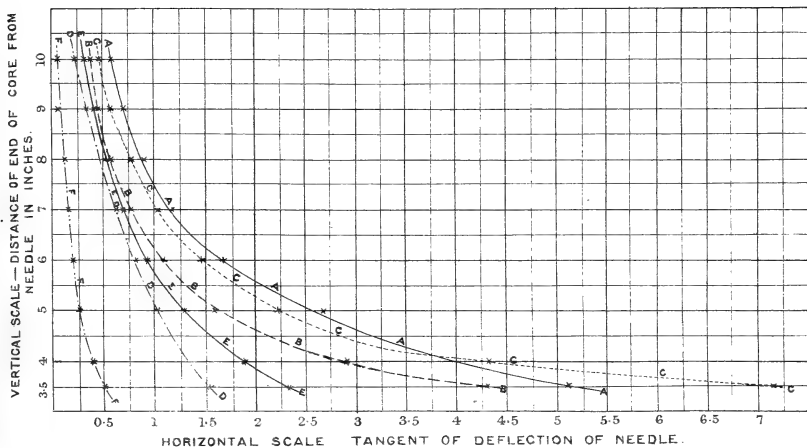
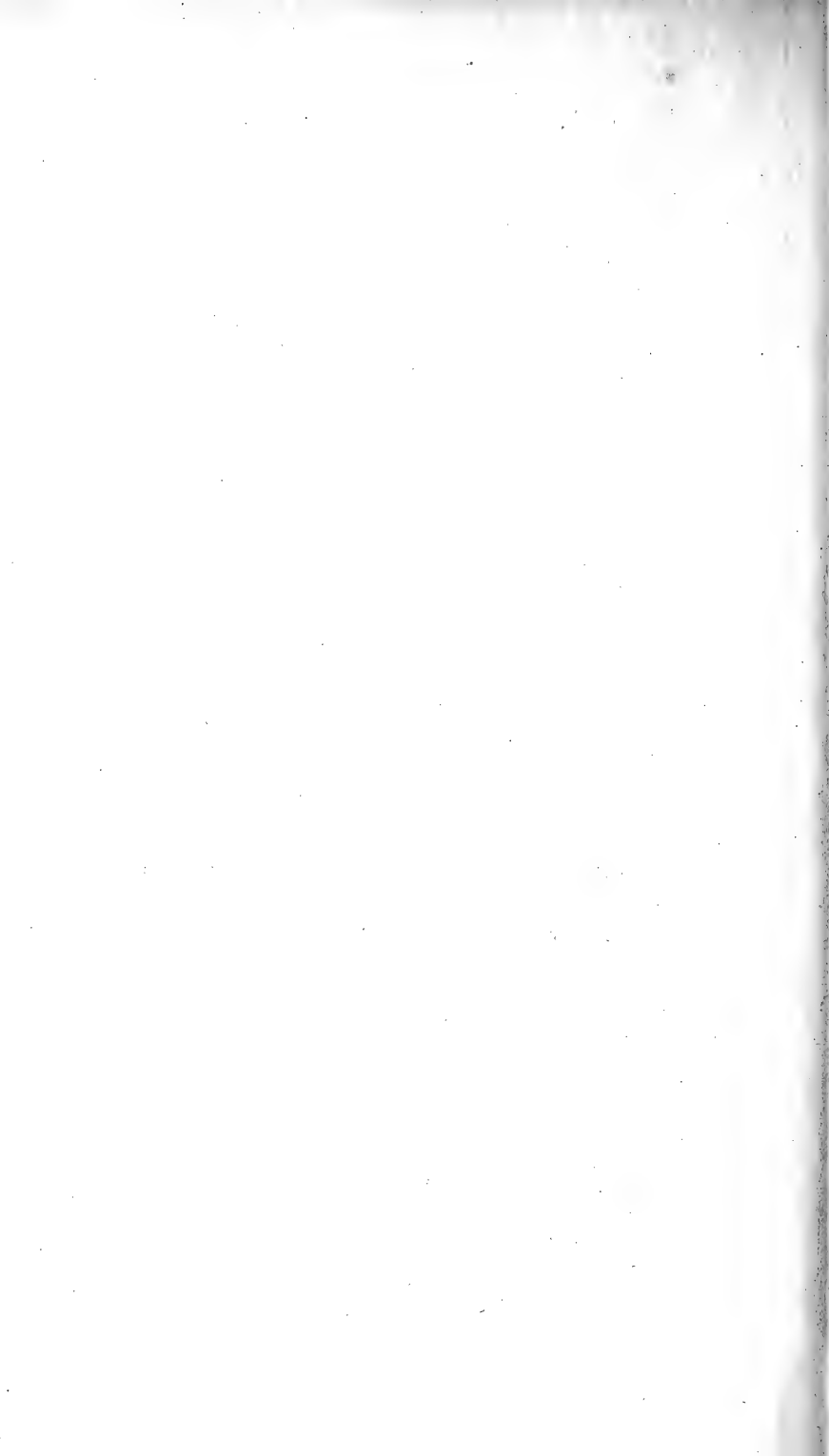
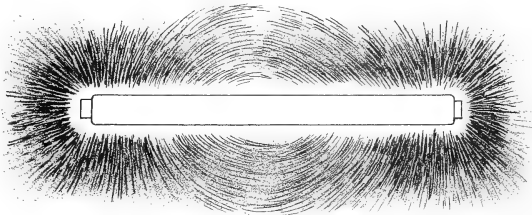


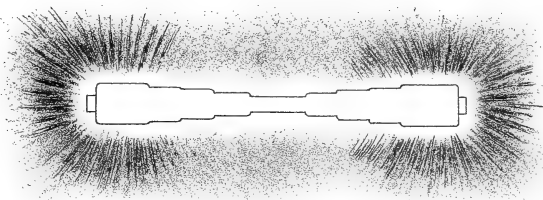
Fig. 5.



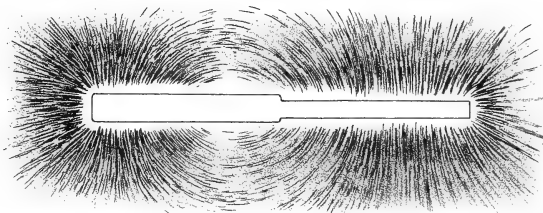
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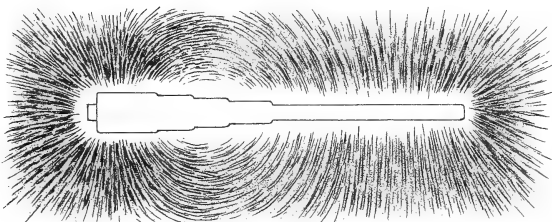
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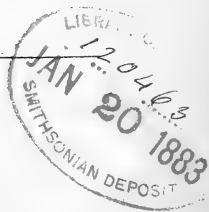
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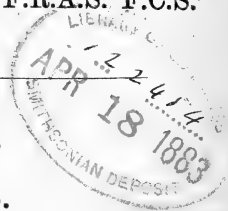
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Page 297, line 32, for $A+u'$ is $A+u'\}&c.$

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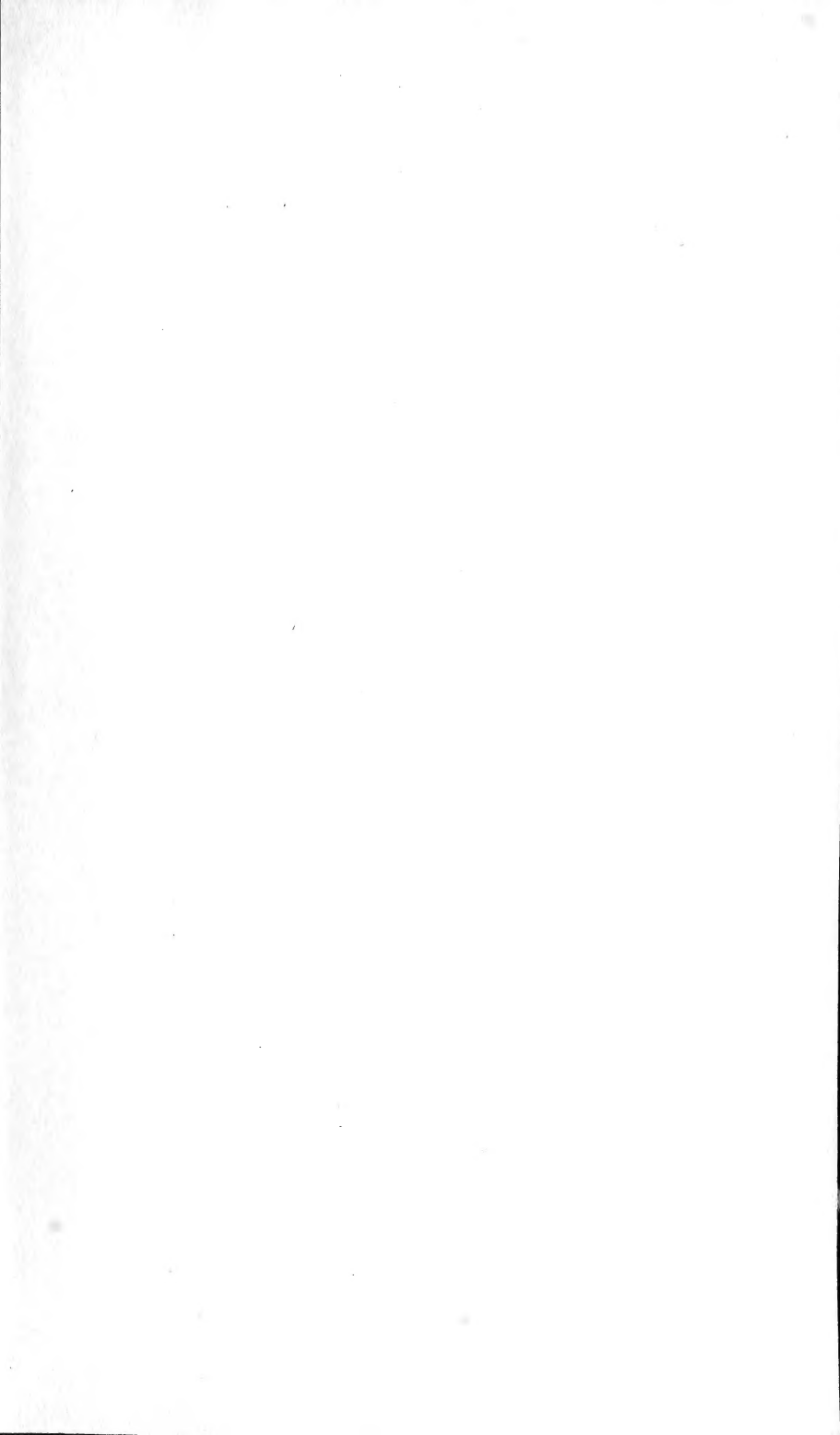
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